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Sociology and Statistics in Britain, 1830-1990

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Declaration

I hereby confirm that this doctoral thesis has been written entirely by myself, is solely the product of my own work unless otherwise specified, and has not been submitted for any other degree or professional qualification.

Signed: Plamena Yankova Panayotova

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Acknowledgements

My interest in the history of sociology and statistics in Britain began when I was still an undergraduate student; at a time when I had more *curiosity* to find out about these subjects than actual *experience* in studying them. Back then, I had been studying sociology for three years but knew little about its development in this country. I had some statistical training but mainly through my studies in psychology. And although I have improved my statistical skills since then, I would still not describe myself as a statistician. I'm not a historian, either – I began reading history of science as a hobby during my undergraduate days and this is how I first came across some of the major works on the history of statistics which gave me the inspiration to delve deeper. My studies really began with my childish knack of questioning everything and demanding answers; but it was stamina, devotion and systematic investigation and a deep desire to emulate the scholarship of those inspirational figures around me that were key in carrying this project through to its end. Now, on its completion, I would like to thank all those people and institutions who have supported me during my research and helped me turn my scattered thoughts into a detailed scholarly work.

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Doing doctoral research is, inevitably, a largely solitary experience that in my case involved many hours of book-hunting, note-taking and imaginary conversations with people long deceased whose deeds and ideas shaped the history I was studying. Thanks to those people, who are too numerous to mention, I woke up on many occasions in a different time, a different year, another century.

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Abstract

This thesis examines the historical relationship between British sociology and statistics in the nineteenth and twentieth centuries. It begins with an analysis of the role that the early development of statistics played in the history of social science, followed by an examination of other nineteenth century, non-quantitative, projects of social enquiry that were to have an influence on the later development of British sociology. The thesis then continues with an analysis of the contributions of the Sociological Society to the development of sociology in Britain and its role in sociology's relationship with statistics. The last and most detailed part of the thesis is devoted to an examination of the trends in the development of academic sociology in Britain in the twentieth century. It analyses the major factors that had significant influence on the possible incorporation of quantitative methods, and a statistical and probabilistic worldview more generally, into British sociology.

Most of the study is based on original archival research and uncovers previously unexamined aspects of events, movements and choices that have defined the character of British sociology since its academic beginnings. The argument is that the relationship between sociology and statistics in Britain has been characterised by a remarkable continuity and been subject to very little change over many years; that it has been distinguished by a negative obsession with statistics on the part of British sociologists who have made consistent efforts to try to prove statistics unsuitable for sociological research and excuse themselves from using them. The study concludes that the relationship between statistics and sociology in Britain has not been determined on the basis of pragmatic concerns but on the basis of uninformed preferences and deficiency in statistical knowledge.

The divide that has existed between sociology and statistics was not inevitable but was the product of a particular set of circumstances and a particular set of choices made, both within and without British academic sociology. The aim of this thesis is to bring to the fore the interplay of these factors and show that the relationship between sociology and statistics matters and ought to be an area of growing concern to British sociologists. It explains not merely British sociology's methodological choices but its relationship with the very thing that made both *it* and

the society it studies – modern science. Ultimately, the relationship between sociology and statistics in Britain matters because discoveries in science in the last hundred years or so have shown that the World, all of existence, social or otherwise, is fundamentally probabilistic; and that statistics is the language best placed to describe how it works.

List of Abbreviations

BAAS British Association for the Advancement of Science

BSA British Sociological Association

ESRC Economic and Social Research Council

HE Higher Education

LSE The London School of Economics and Political Science

OECD Organisation for Economic Co-operation and Development

RSS Royal Statistical Society

SSA Social Science Association

SSaA Social Science and Administration

SSL Statistical Society of London

SSRC Social Science Research Council

UGC University Grants Committee

Table of Contents

Chapter One

Introduction

Throughout the twentieth century, British sociology has been obsessed with statistics. Obsessed not in the sense of yielding to excessive use of statistical techniques and thinking, but obsessed in the rather negative sense of constantly needing to prove the unsuitability and the potentially harmful results of statistics in order to excuse itself from using them.

Historically speaking, when British sociologists talked of 'statistical *obsession*' they meant *other* social scientists' overuse of, or unquestioned belief in, statistics both here and elsewhere, notably the USA (see Chapter Eleven). Unsurprisingly, they have been reluctant to recognise their own repetitive, inconclusive and obsessive reflections on the place of statistical methods and thinking in social science and on whether or not statistics has a place in sociology at all. Could it be the case that British sociologists' reluctance to recognise the existence of such an obsession is indeed justified and that the very idea of it is merely an historiographical fiction? In the same way in which a statistician who is intentionally looking for patterns and significant results in their data is bound to find *some,* in studying an issue like this from a predominantly historical viewpoint, isn't there always the danger that we find in that history precisely what we are looking for?

A primary aim of this thesis is to show that the relationship between statistics and sociology – the negative obsession of British sociology with statistics – has its own historical reality which is independent of any historiographical choice. Moreover, it aims to show that the questions that the examination of this relationship raise have a genuine historical importance that goes beyond any concerns that exist in sociology in Britain at present.

This thesis further aims to explain how this obsession originated and what sustained it through the numerous changes that took place over many decades. It looks firstly at the conditions which brought statistical methods and statistical thinking to the forefront of social science in the nineteenth century and from which British sociology, eventually, emerged at the beginning of the twentieth century (Part

One). It then examines the changing character of British sociology throughout the twentieth century and reveals the factors that played a vital role in determining that character and sustaining its peculiar obsession with statistics (Part Two and Three).

The analysis follows a chronological order but the general themes explored are not limited to any one particular period; rather, I aim to draw attention to the fact that in spite of all that changed in British sociology, the nature of its relationship with statistics has been characterised by a remarkable continuity and consistency – and this is precisely why examining this relationship gives us a unique vantage point. On the surface, this is a story about a nineteenth-century struggle to obtain reliable knowledge about the world amidst an unprecedented rate of social change and intellectual shifts; about the noble aspirations of a Sociological Society which brought sociology into existence in Britain but left it a wandering orphan; about intellectual quarrelling and institutional settlement; about a remarkable expansion of the higher education system that turned out to be as much of a hazardous challenge for sociology as it was its long-awaited blessing. On a deeper level, however, this is a story about British sociology's response to the rise of modern scientific thinking; about its indecisiveness and its continuous self-questioning about its character and aims, played out against a background of suspicion of statistical thinking; and about how the power of attitudes and irrational convictions has prevailed over rational considerations at crucial moments. In the end, it would appear that British sociology's negatively charged obsession with statistics is what defines it better and more clearly than anything else.

Why Study the Historical Relationship between Sociology and Statistics at all?

Anyone who has experienced the more recent relationship between British sociology and statistics, will be aware that this relationship has been and remains largely unproductive and while this has been recognised in some circles (British Academy (2012), Byrne (2012), ESRC, BSA and HaPS (2010), MacInnes (2010) May (2005), Payne et al (2004), Williams et al (2004a, 2004b) and Williams et al (2008), Goldthorpe (2015)), a solution that reaches all sectors of sociology, not just

those interested in 'statistical sociology', has not been found. Quantitative methods have not been totally absent from the sociological curriculum but, nonetheless, the effective incorporation of these methods into sociological teaching and research in Britain has recently been limited, despite their increasing importance in a digitised and data-oriented world in which the study of human behaviour, including social human behaviour, shows promising potential.

Although the present study may help explain what made a situation like the current one possible, it also shows that this course of development was not inevitable. And although it aims to illustrate that the relationship between sociology and statistics in Britain has developed along a particular long-term evolutionary course, it acknowledges clearly the different contexts in which this evolution has manifested itself. An historical study like the present one can therefore broaden our understanding but it cannot and does not on its own provide a ready solution to any situation that we may perceive as problematic today; historical analysis does not create or reveal laws with predictive or healing power simply because the events, situations, contexts of the past are rarely, if ever, replicated in the present or future (for a more detailed discussion, cf. Evans, 1997).

Broadening our understanding of a current affair, or acting upon some present concerns regarding the education of the next generation of sociologists in a data-driven world may be a sufficient justification for an educational or policy-related study of the relationship between statistics and sociology; but an historical study requires a more fundamental and wide-reaching justification. An historian, in other words, must be able to show that their study is important regardless of any present conditions: the fact that some sociologists in Britain have recently come to grapple with what they perceive as a problematic relationship between sociology and statistics makes a study like the present one relevant but it does not automatically make it historically significant. So where does the historical justification and significance of the present study lie? Why study the historical relationship between sociology and statistics *at all*?

There are some general benefits that come from an historical enquiry that also apply to the present study. While some of the conclusions of this study may not be surprising in themselves, the question of what, historically, influenced the

relationship between statistics and sociology, has not been asked or examined before. The novelty and originality of a piece of research comes as much from the novelty of the research questions as from the novelty of the answers and those who raise objections against this whole work based solely on the lack of 'surprising' conclusions are simply revealing a lack of appreciation for the value of empirical historical research; an attitude which I, in fact, show to be characteristic of British sociology since its beginnings. Thus a simple answer to the question 'Why study the historical relationship between sociology and statistics? is that this question has been left neglected and unexplored in scholarship.

Another benefit from approaching a question historically is that it creates an opportunity to revise and modify existing knowledge. This thesis is not only looking at the development of sociology from a new angle; it also reveals in what ways some of the most cited accounts of the history of British sociology need to be modified. Abrams' 1968 essay is one of many examples that I will discuss: it has helped spread the view that the nineteenth-century development of statistics in Britain frustrated the development of sociology. As I show, a closer and more contextual examination of the evidence not only reveals the flaws in this argument, but, beyond this, it reveals something more fundamental about the nature of British sociology; for Abrams' reasoning has also been post-war sociology's reasoning. It is as if Abrams and others wrote their interpretations from inside sociology looking out, while my aim has been to write from the outside looking in. But this thesis does not merely aim to show that an alternative perspective on British sociology and its history is possible; it aims to show that it leads to a better grounded and more historically accurate account of the relationship between sociology and statistics that has the power to reveal the deeper and wider implications of this relationship for sociology as a whole.

History's lack of predictive power does not mean it cannot provide us with examples of something similar, useful analogies that aid us in our assessment of the current situation – it is important to remember that 'the impossibility or low feasibility of prediction does not exclude the possibility of comprehension' (Gellner, 1988: 15). Without historical knowledge, it would be very difficult, if not impossible, to establish what is different and unique about the current situation. History may not help us predict the future but it can open our eyes to *alternative* possibilities, past

and future. The present thesis therefore, is intended to provide relevant and indispensable evidence to assist the process of assessing not only the direction that British sociology has taken in the past but also in seeing the opportunities, the alternative possibilities, that lie ahead.

The novelty of the research question, the need to modify existing historical accounts and the opportunity to make detailed analysis of the historical evidence that will allow us to broaden our general knowledge all contribute to the scholarly value and historical importance of the present study. However, there are more specific, and in some ways more fundamental, reasons why an historical examination of the relationship between sociology and statistics in Britain is justifiable that relate to specific characteristics of history, British sociology and statistics.

History

An historical examination of the relationship between statistics and sociology conducted in the twenty-first century does not necessarily have to fall into the trap of 'presentist' (also known as 'Whig') interpretations of history and can actually improve our understanding.

The Whig interpretation of history was first identified, and condemned, by Herbert Butterfield in 1931. It is generally characterised as placing emphasis on the similarities and analogies between past and present (Butterfield, 1965 [1931]: 10) and by extracting things from out of their historical context and judging them in relation to present concerns (Butterfield, 1965 [1931]: 30). It upholds the present as absolute against which all things past are to be measured in order to reveal who is in the right, as opposed to showing how people and ideas came to differ over time (Butterfield, 1965 [1931]: 16, 30, 130). The danger of this approach is that, either intentionally or unintentionally, the historian distorts the past by assuming that something they hold to be true today should guide their judgement and understanding of past events. Sociologists who have studied the history of British sociology have already fallen victims of 'presentism', the most obvious example being Abrams' 1968 essay on the origins of British sociology. In this essay, Abrams imposes his

understanding of sociology as a primarily theoretical intellectual enterprise (as opposed to empirical or policy oriented social research) on the nineteenth-century history of social science, which leads him to interpret any movement or person that did not have his understanding of sociology as harmful and obstructive to the potential emergence of the kind of sociology that Abrams believed to be the best possible sociology. This is one way of interpreting the nineteenth-century history of sociology in Britain but, as is usually the case with presentist accounts such as this, it tells us more about the way sociology was understood in the 1960s, when Abrams was writing, than about the ways in which social scientists themselves understood what they were doing in the nineteenth century.

Presentist accounts are also characterised by an emphasis on the continuity of historical development – another example of such an historical account, albeit one that rests on much better evidential basis than Abrams' – is Renwick's (2012) book on the biological roots of British sociology. In attempting to present the formation and early history of the Sociological Society as a culminating point in the overall history of British social science, and also as a point at which the development of a biological sociology in Britain was severed, Renwick's account provides an oversimplified interpretation of the events in British sociology towards the end of the nineteenth century. The present study is necessary, therefore, not only as a means of modifying existing knowledge and providing a different interpretation but as a means of showing that existing accounts *can* be improved.

To do so, I follow Butterfield's advice that 'the chief aim of the historian is the elucidation of the unlikenesses between past and present' (Butterfield, 1965 [1931]: 10); and that what should guide the historian in any historical examination is 'the belief that we can in some degree enter into minds that are unlike our own' (Butterfield, 1965 [1931]: 9). Following Butterfield in his belief that it is possible to write history in a non-presentist way, this study emphasises changes in the contemporary social, political and scientific context of nineteenth-century statistics; changes in the understanding of science; changes in the organisation of higher education and its rapid expansion in the post-war period. It also pays special attention to the variety of historical contexts in which the relationship between sociology and statistics developed - an historical examination is more fruitful, and

faithful to the past, when is consists of detailed and contextualised examinations of particular events, processes or movements rather than attempts to construct a general overarching explanation connecting all examined events or movements. This, of course, is not to say that we should ignore the degree of continuity where it exists between one set of events and another but it is equally important to recognise where the links are missing.

However, to produce an historical account of the development of sociology it is not enough to commit to the general principles of non-presentist history writing. A good piece of history that has the potential to improve historical knowledge does not imply a total rejection of the use of the benefit of hindsight and of the possibility of finding long-term pervasive trends in history; on the contrary, it aims to strike a balance between interpretation based on detailed contextual analysis and interpretation based on hindsight thinking. Without the benefit of hindsight there would be no clear way of deciding what is of historical significance and what to study in the first place. Thus, it is only through the benefit of hindsight that we could recognise the potential and long-lasting effectiveness of statistics in studying both the social and the natural world, and, consequently, consider it important to study its historical relationship with other subjects. This, of course, is different from assuming that because we can see statistics as important *now*, statistics was always seen as important. A balance between continuity and change is necessary and can only be achieved by taking into account both past and present; by reading history both backwards and forwards.

There is another reason why the benefit of hindsight is useful in history writing – we can compare the intentions of a person or an institution when they chose to act in a particular way with the consequences of their actions which we, with the benefit of hindsight, know, but which they could not possibly have known with any certainty. For instance, the Sociological Society was founded with intention to unify all existing social science specialisms into the general science of sociology, to set up a sociological journal and establish the subject academically. However, if in our study of the Society, we were only to focus on their intentions, without recognising that the consequences of their efforts fell short of fulfilling their intentions, our account would be extremely limited. This is one way of examining a particular event, in this

case the work of the Sociological society, using both their understanding of their work, as manifested in their intentions, but also our understanding of their work, based on their legacy. Therefore, although Butterfield is correct is arguing that 'to assume the present at beginning of our study' can potentially lead to distorting accounts, experience shows that he is not correct in equating this with the keeping of the present 'as a reference' (Butterfield, 1965 [1931]: 62). In fact, only by keeping the present as a reference could we begin to recognise the differences between past and present that Butterfield says are so important when we write history.

A good piece of historical analysis also acknowledges that history is not entirely the product of deliberate decision making. In Butterfield's words, history is not only a product of 'agency', but also of slow and incremental 'processes' (Butterfield, 1965 [1931]: 50). This is another criterion which existing accounts have failed to adequately satisfy and on which the present study aims to improve. As I will show, the difficult historical relationship between sociology and statistics throughout the nineteenth and twentieth centuries was not the result of anyone's decision-making or careful planning.

But something fundamental that even Butterfield does not mention, is that history is also not entirely a product of *rational* choices. It is a well known principle, for instance, that the research methods sociologists use to study a particular problem should be determined by the research question at hand and that all methods are, in principle at least, equal. Although few would disagree, what we see in practice is that some methods are 'more equal than others' and that the choice that sociologists make regarding what research methods to use is rarely determined solely by their research question. Moreover, sociologists can abstain and have abstained from asking particular questions because that would involve using research methods they do not favour. It is not always possible to explain fully why the level of receptiveness to certain methods, or ideas more generally, has varied with time, especially when talking about ideas that are well received in our time but were less well received in the past. But it is important to try to explain what drove the presence or absence of receptiveness; and, I argue, that while sociologists did not make a rational choice or take a deliberate decision to restrict British sociology's engagement with statistics, the difficult relationship between the two subjects resulted from the effects of a

series of interrelated beliefs and opinions about statistics that spread and evolved from one generation of sociologists to the next. And, without apportioning blame or making judgements, the statisticians themselves, it seems, did little to try to turn these beliefs and opinions around. Further justification of why a study of the relationship between sociology and statistics in Britain is not only an alternative but also better account than existing ones will be provided below with particular reference to the role of statistics in the development of modern scientific thinking.

Sociology

Given that an historical study of the relationship between statistics and sociology is justified through the possibility and necessity to improve existing historical knowledge, are there any particular reasons why a study of this kind should focus on the British case?

It is well known that the development of social science in Britain has been characterised by a peculiar divide between academic sociology and what is generally known as the British empirical tradition of social enquiry that developed in other academic departments and civic and government agencies. Bulmer among others has argued that it is 'hardly an exaggeration to say that almost all of the empirical social research undertaken in Britain between the wars went untouched by what then passed for academic sociology' (Bulmer, 1985: 4). This thesis provides further evidence that throughout the nineteenth and twentieth centuries the empirical tradition of social enquiry has included most of the statistical research done in Britain, while academic sociology has *consistently* been oriented towards philosophical and theoretical work. The difficulty in understanding these two parallel traditions is not resolved by treating one as a subset of the other; we can achieve deeper and better understanding only by acknowledging the division and analysing what sustains it.

Since the application of statistical methods and thinking in social science has been mostly done within the empirical tradition of social enquiry, and not within academic sociology, an historical examination of the relationship between sociology and statistics is justifiable as it offers a suitable opportunity to find out why British

sociology as a whole has developed in this particular way and why British social science more generally has been split, *given that this did not necessarily have to be the case.*

Evidence for this comes from the experience of other countries, such as the USA, where historians have observed the introduction of statistical training and thinking into American sociology towards the end of the nineteenth and beginning of the twentieth centuries as a result of particular academic and institutional conditions. The fact that this integration occurred early on in the USA serves as a possible example of how a statistical sociology can emerge; but it on its own cannot explain why this did not happen in Britain, nor is there any guarantee that, had Britain had similar higher educational institutions, a statistical sociology would have emerged here. It might be thought that the development of more sophisticated statistical techniques, many *originating* here in Britain, would have created especially favourable conditions and stimulated the growth of a quantitative social science; this, however, proved not to be the case. Knowing that a statistical sociology was nonetheless possible, even in the USA which did not have a distinctive statistical tradition as in Britain, prompts us to reconsider traditional explanations and search deeper for reasons that go beyond the simple availability of statistical knowledge to sociologists. A comparative perspective is therefore illuminating in the sense that it opens up our mind to alternatives and draws our attention to the peculiarity of the conditions that existed in Britain and the importance of examining them more closely.

For the purpose of examining the divide which exists within British social science, in this study, by 'sociology' I mean the subject as it developed academically in Britain and was taught by sociologists in departments in higher education institutions throughout the twentieth century, referring to the rest of the social science field as simply 'social science', or social science and administration or the empirical tradition of social enquiry. I use the term 'sociologist' for those scholars, teachers and researchers who called themselves 'sociologists'. Those who were doing research in other areas, such as social policy or social administration, are referred to as 'social scientists'; and those working in the field of social statistics, as 'social statisticians'.

I realise that there are certain drawbacks in limiting our understanding of sociology only to the work done by those who called themselves sociologists. This is a common problem for scholars studying the history of social and natural science. For instance, the words 'science' and 'scientist' came to be used to describe what we today understand as science only in the 1830s (Ross, 1962) but of course this does not mean that there was no science or no scientists before this time – on the contrary, we can see a clearly identifiable 'scientific' approach to investigating the natural world from the early seventeenth century onwards. But the story of the word 'sociology', and what preceded it, is different from the case of 'science': in the case of 'science' and 'scientist' a new word was invented in the 1830s to describe the *same* thing that was previously described by words such as 'natural philosophy'; in the case of sociology, the word and title were introduced by Auguste Comte in the 1830s to describe something different and separate from other nineteenth-century projects of social enquiry in Britain, such as social reformism or social statistics. Moreover, this difference persisted throughout the twentieth century at an institutional level, with separate departments and research centres being established for sociology and for other types of empirical/policy-oriented research (one exception is Nuffield College); and at an individual level, i.e. rarely did the same people work in both traditions, with the exception of, for instance, David Glass or A. H. Halsey. It is, therefore, historically justifiable to distinguish between sociology and the other social empirical traditions of enquiry on the basis of what they called themselves because the different names had clear divisions.

An additional historiographical approach that facilitates the historical examination of the divide within British social science centres upon a distinction between the *institutional* development of academic sociology and its teaching and the purely intellectual development of sociological ideas. In this study, I emphasise the former more strongly than the latter. This, however, does not mean that I completely ignore the intellectual development of the subject and the influence that individuals such as Auguste Comte, L. T. Hobhouse, Morris Ginsberg, T. H. Marshall etc. exerted on the development of British sociology. But in order to understand how they came to hold particular ideas and beliefs, how and why they sustained them and what influence they had on sociology more generally, we need to

examine the institutional context in which these ideas circulated. Focusing on teaching allows us to see what ideas and conceptions of sociology came to form the core of academic sociology in this country and were used to train subsequent generations of sociologists, thereby strongly influencing their views of what sociology was about and what it was going to be about in the future. Where particular scholars have engaged with statistics alongside their sociological work, this has been examined in more detail with a view of shedding light on how this was done and under what conditions and whether or not it had any effect on the more general incorporation of statistics into sociology.

Statistics

Providing justification for approaching the topic of the present study from an historical viewpoint and also in choosing to examine, specifically, British sociology leaves open the question of whether there are any reasons for examining its relationship with *statistics*. Of what historical importance, therefore, *is* the relationship between British sociology and statistics?

To answer this question, first, a distinction has to be made between quantitative research methods and statistics. By quantitative methods in this study I mean the mathematics and procedures of quantitative data collection and analysis that are taught to social science students in the classroom.

Statistics, however, is broader concept. Among statisticians there is a common understanding that statistics is a general set of tools:

> …it creates a structure for scientific exploration. […] So it is much more like a set of methods but it's a set of methods that are rooted in trying to describe scientific reality. I think to describe it as a technology is the nearest as one gets (Author's Interview with Harvey Goldstein, 2017).

> [statistics is] the principle instrument yet devised by man for bringing within his grasp the terrifying complexity of things and relations between things and as a powerful illuminant of rational thought itself (Kendall, 1950: 127).

But statistics is not merely a technical tool – it does not merely include mathematics, probability and modelling, although this is essential. It also involves a statistical way of thinking, or worldview, which is characterised by the ability to think and analyse on a larger scale and to understand and work with probabilistic knowledge not only when conducting statistical analysis, but more generally, as an intelligent citizen. To have a statistical worldview about the social world would at the very least imply an acknowledgment that society is, at its core, a statistical entity. Thus being statistically skilled does not merely mean knowing, for instance, how to collect survey data or conduct a regression analysis – statistical output on its own does not provide *explanation* of social phenomena. Being statistically skilled also involves ability to use statistical output imaginatively, in a way in which one is able to provide more accurate, reliable and exhaustive explanations than existing ones. And it is here that appreciation of the importance of the statistical nature of society is vital – any explanation that does not take this into account is bound to miss an important point.

Once we start thinking about statistics as a *conceptual* as well as a *technical* tool for conducting empirical analysis, it becomes clear how misleading it is to think that statistics is merely about numbers and that the relationship between statistics and sociology can be boiled down to teaching sociologists some mathematics. Those who believe that statistical research is mere number crunching and that it is *mainly* about numbers should think again. Although there is social empirical research that contains only words, *there is no piece of social empirical research that contains only numbers*. The true value of an empirical piece of research that contains a collection of statistical data and uses statistical modelling to analyse these data lies not in the statistics themselves but in the *explanation* of these statistics that draws on sociological concepts, premises or theories and goes *beyond* the data. This is not to say that empirical social enquiries which lack a quantitative element are incapable of providing powerful and reliable explanations of social phenomena; they are, but since generalisation is much more difficult in these cases, without a quantitative element, it is much more difficult to say something meaningful and, at least, approximately true about society as a whole.

Paying special attention to the relationship between sociology and statistics therefore can be justified on the basis that the statistical and the sociological approaches to studying society are, in principle, not only compatible but *necessary* for each other. A clear explanation why this is the case has most recently been provided by Goldthorpe (2015). According to Goldthorpe, although statistics is necessary and fundamental for data collection and data analysis of probabilistic regularities, 'statistical analysis alone cannot lead to causal explanation of these regularities' (Goldthorpe, 2015: 103-4). What this means is that statistical models showing, for instance, the effect of one variable on another, cannot be taken as final results – they can provide evidence that there is a relationship between variables but they cannot in themselves explain how this relationship has come about. To explain this we need a theoretical input in the beginning or theoretical output in the end, say from sociological concepts and theories (for good examples of how this could be achieved in practice, see Goldthorpe 2015: 115-119). The fact that this can be done, however, does not necessarily mean that it is easily done and it is a prime aim of the present study to show what factors were involved in the history of British sociology that made it more or less likely to accept and act upon the potential compatibility between statistical and sociological conceptions of society.

Statistics as both a conceptual and technical tool could contribute to a better sociological understanding which is another reason why it is important to study the mutual interaction between the two. For instance, statistics is essential in the study of wider trends which are the product of unprecedented rate of change and rapid dynamics which not only affect but *define* modern society. It may be the case that we need more focus and smaller scale studies using qualitative methods to examine how these changes are affecting individuals in their everyday routine or in their emotional life, but without a clear view of the general trends underlying these effects, the usefulness of qualitative studies of this kind is greatly diminished.

Statistics can also contribute to a better understanding of relationships between sociological phenomena. No other method enables us to measure the degree to which one phenomenon is related to another, including the ability to make generalisations that apply to entire populations.

There are also ways in which statistical knowledge can be beneficial even in the case of non-quantitative sociological analysis. One of my interviewees explained his attempt to teach his students basic statistical principles in the following way:

> I would say to them, you may not like numbers and you may or may not think you are very good at them but there are lots of clever people out there who are going to use numbers systematically to mislead you [...] If you don't acquire some capacity to handle numbers with a degree of confidence, you are going to be a victim, so for heaven's sake, lets get on top of this and learn how to appreciate the numbers [...] So I would say to students, this is in a sense nothing to do with sociology, it's to do with citizenship and being a member of society (Author's Interview with Robert Moore, 2017).

Statistics teaches students to pay careful attention to the quality of data, *any data*; guides them in judging the extent to which they can draw firm conclusions from their data; allows them to measure the inherent variability and uncertainty in any sort of findings about the social world and teaches them the importance of precision in thought and writing. This is not to say that training in other methods necessarily lacks these elements; but while in statistical training these issues are prominent and widely discussed, teaching of qualitative methods usually focuses on other elements such as interpretation and the application of theory.

The practical benefits that arise from acknowledging, in principle and in practice, the potential compatibility of statistical and sociological approaches to studying society, however, provide only partial justification for looking at the relationship between the two subjects which, it is not difficult to imagine, can be contested by sociologists who believe that sociology and statistics are *epistemologically* incompatible (examples of such arguments will be discussed in Chapter Thirteen). Are there any potentially more fundamental ways in which an historian can justify examining and even questioning British sociology's historical relationship with statistics? Does statistics matter *beyond* the practical benefits it could bring?

Statistics as a quantitative method has evolved mathematically and technically throughout the years to make possible more accurate and more powerful statistical data collection and analysis. This, however, is only one way to look at the historical development of statistics, of which Stephen Stigler's works (1986, 1999, 2016) are a

prime example. Alongside the technical development of statistics there is another aspect of its historical development that is especially relevant when studying statistics in the context of the history of social science and has to do with the role of science in modern society and the important role statistics has played in scientific advance since the early nineteenth century.

The nineteenth century witnessed the formation and rise of the sciences of geology and biology as well as major advances in the fields of chemistry and physics. Scientific discoveries led to advances in technology which over time contributed to the re-shaping of social and political structures and to dramatic changes in the way people lived their daily lives. Statistics, as we know it today, did not initially develop as a method to aid investigation in the natural sciences; it first developed as a method aimed at improving the study of society which was only later adapted to the study of natural phenomena. But, as I will show in Part One, from its very beginnings, the principles behind statistical analysis adopted the characteristics of nineteenth-century science. In this way, statistics was one of the representations of the consolidation of a new type of 'culture' in the Western world – a culture grounded in reason, rationality and empirical principles for the establishment of reliable knowledge; a culture defined by science and the *power* of science (Gellner, 1992). Therefore, statistics represents a particular worldview, a style of reasoning which has played an important part not only in providing a basis for the development of all statistical techniques, but also in shaping the fundamental concepts in both social and natural science. Moreover, even before statistics embraced the advances made in the mathematical theory of probability and statistical explanations and statistical modelling became available for the use of social and natural sciences in the late nineteenth century, statistics provided a concept of society (and of nature) that fundamentally changed the way we think about and approach these entities – this was not a concept of society that was based on abstract principles of the forces within society (as in the Hobbesian principles of the seventeenth century or the ideas of social contract of the eighteenth century) or on abstract theories of how to improve society (as in nineteenth-century Marxism). The statistical concept of society was based on a set of procedures on how to analyse social phenomena in a more effective way than had been done before and how to derive knowledge from the empirical

investigation of society; it provided a way, in line with the scientific way of investigating nature, of testing, of scrutinising all existing knowledge about society. It was in this sense that nineteenth-century statistics *made social science possible – the development of statistics initiated not only a new way of doing research into society but it established the foundations of social science itself.* A quantification mind-set had emerged which, unlike what is usually said about social statisticians, was not just about counting up everything that could be counted but about ways in which reliable knowledge about society could be achieved.

Britain was at the forefront of all these changes and therefore makes an ideal testing ground for understanding how sociology, which, as I show below *was* only one of a variety of social scientific enterprises, responded to them. At its core, this thesis is essentially about the factors and conditions and that led to the emergence and development of a sociology in Britain that largely ignored, and occasionally rejected, the thing that made social *science* possible.

Once we begin to understand statistics not merely as a method but see how it is inextricably linked to a way of viewing a world that is inherently uncertain and variable, we see that a study of the relationship between statistics and sociology can reveal something important about British sociology more generally. *It relates to a bigger question about how the study of society in Britain, and sociology as one part of it, responded to the changes in the cultural and scientific outlook that emerged in the nineteenth century. As I show in detail later on, how sociology responded to statistics is how sociology responded to modern science.* By the nineteenth century the scientific method had proven itself and it had made possible the discovery of new knowledge that correlated with the way the universe actually was better than anything else discovered or invented before. The foundations of all statistical thinking which were laid in the nineteenth and twentieth centuries were part of that same outlook that brought about major advances in the study of electrical phenomena, Darwin's theory of evolution, Maxwell's laws of thermodynamics quantum mechanics – all of which, to a greater or lesser extent, implicitly or explicitly, relied on statistical principles. It is only through the benefit of hindsight that we know how fundamental nineteenth-century statistical thinking was in helping to bring about discoveries and inventions that changed our world beyond recognition.

Contemporary British sociologists could not have known the full, long term implications, but at a time when the successes resulting from the statistical approach were building, the fact that British sociology, whether by choice or circumstance, took a direction different from that of the general development of natural and social science clearly deserves further investigation. Examining the relationship between sociology and statistics is therefore ultimately justifiable at a much deeper level than simply the benefits arising from sociologists' use of statistics – the level of the relationship between sociology and science a system of knowledge that defines the very object of sociology's study, modern society.

In our *post*-modern days, however, even the ability of science and history to bring about advancement in knowledge has been questioned. Since the 1960s, there have been numerous charges made against the possibility of objective scientific or historical knowledge coming from post-modern and post-structural philosophers and sociologists (Foucault, Haraway, Derrida, Bloor). Some have argued for extreme relativism, basing their view on the understanding that language could not relate to anything but itself; that everyone's point of view is equally valid; and that historical but also natural and social reality are all socially constructed and are being constantly re-constructed (Evans, 1997: 1-14). Some historians have objected to such arguments aimed at history by arguing that:

> It does not follow that, because a mountain appears to take on different shapes from different angles of vision, it has objectively either no shape at all or an infinity of shapes. It does not follow that, because interpretation plays a necessary part in establishing the facts of history, and because no existing interpretation is wholly objective, one interpretation is as good as another, and the facts of history are in principle not amenable to objective interpretation (Carr, 1974 [1961]: 26-27).

Others, like the historian of science David Wootton, have been adamant that post-modern arguments aimed at discrediting science are fallacious and totally missing the whole point of science, since 'science as a system of knowledge is more than a social construct because it is successful, because it fits with reality'; if contemporary methods of science fit better than older ones, it is because they fit 'better with the world as it is, not because the world was bound to be like this' (Wootton, 2015: 540). I tend to agree with Carr and Wootton and hold that none of

the post-modern critiques against history or science can successfully invalidate or even challenge the ultimate justification of examining the relationship between sociology and statistics, as outlined above.

Post-modern critiques of science and history amount to a proposition that knowledge is merely a collection of equally valid viewpoints and that better or more useful or more adequate understanding is not possible. However, this in turn means that knowledge itself is impossible since the very idea of knowledge is about better and more adequate understanding than what we had before. To reject the possibility of knowledge is, therefore, also to reject that post-modernism itself is knowledge and that it has something better to say about how we can learn about the world. Post-modernist thought, therefore, contains within it the seeds of its own invalidity. Moreover, in arguing for extreme relativism, that all knowledge is relative and therefore valid, post-modernism would automatically give equal status to the objectivity argument. It is, therefore, also self-contradictory. As far as objectivity is concerned, post-modern ways of thinking, it seems, has nothing useful to say.

Similarly, although the interpretation of a social phenomenon may vary from person to person, from place to place and from one point in time to another, post-modernist thinkers fail to recognise that *in its consequences* the social world is *as real as* the natural world. There can be numerous social causes and explanations for poverty but, ultimately, in its consequences on people's mental and physical health, poverty is just as real as, for instance, the natural erosion of rocks. Social phenomena may be socially constructed in their origins, but they are not purely socially constructed in their consequences. Objective accounts in social and natural science are, therefore, a legitimate aspiration – they are those accounts which allow us to grasp more comprehensively and to understand more deeply the consequences of natural and social phenomena. One way of achieving this is through the application of scientific methods and principles, a prominent part of which are quantitative methods. It is through the incorporation of such methods that we can know with better precision and accuracy, with more depth and clarity what we understood yesterday less precisely, less clearly and in only a superficial way.

The fact that both natural and social scientists can legitimately strive towards objective knowledge does not imply that there are no differences between the natural

and the social world and the approaches to studying them. What distinguishes social from natural science is **not** the fact that natural science is somehow more capable of producing objective knowledge based on the gathering and analysis of factual evidence, while social science, by virtue of studying conscious and language-using human beings, is not. What distinguishes the two enterprises is that, first, the number of ideas in social science is limited – what is being said in social science nowadays has mostly already been said at an earlier point by another person in a different way. In contrast, natural science contains a potentially infinite pool of new ideas and has the ability to produce new knowledge, make new discoveries and invent new things. Therefore, knowledge in the realm of natural science is cumulative in a way in which social scientific knowledge cannot be cumulative (cf. Gellner, 1985: 7-9) and the latter cannot have the impact on our cognitive world that natural science has by enriching it with new knowledge and expanding it to new territories (cf. Gellner, 1985: 126-7). It is a mistake, however, to use the non-cumulative character of social science, as an argument against the possibility of objectivity. Social science may not be cumulative in terms of how much we know; but it too progresses from less to more sophisticated ways of knowing the social worlds due to the progress in the scientific methods of study that we employ in it.

Absolute objectivity and, with it, absolute progress, might be an illusion; but objectivity *can* still be used as a criterion in social and natural science and for evaluating historical accounts. The concept of objectivity in both history and science is analogous to the statistical concept of 'moral certainty' – moral certainty refers to a probability that falls short of mathematical or absolute certainty but that, nonetheless, is high enough to allow us to act upon it. Writing a *totally* objective or totally definitive historical or scientific account is just as impossible as it is impossible to know something with absolute certainty. But just as the impossibility of absolute certainty does not stop statisticians from producing accurate, reliable and new knowledge, the impossibility of total objectivity does not preclude making scientific advances and doing better history in a more advanced way, thereby producing more accurate knowledge and a better understanding of the society, nature and their past.

An historical examination of the relationship between statistics and sociology in Britain, therefore, is not only legitimate and valuable in its own right but it is also of substantial historical significance. It allows us not only to advance our historical understanding by revising existing historical knowledge on the basis of a more contextualised and evidence-based approach; it also allows us to examine some of the perennial issues in British social science more generally, such as the divide between empirical social science and academic sociology. And, ultimately, examining the historical relationship between sociology and statistics will shed light on how sociology in Britain has dealt with the scientific advances, part of which is statistics, that created and sustain and define better than anything else our modern society. How science and statistics achieved this and how the particular idea of sociology emerged amidst the rise of science and statistics in the nineteenth century is the topic of the first part of this study.

PART ONE

SOCIOLOGY AND STATISTICS IN NINETEENTH-CENTURY BRITAIN

Introduction

In Britain, in the nineteenth century, there were no 'sociological' institutions and no professed 'sociologists'. Auguste Comte, who invented the term 'sociology' to describe his particular idea of a positive social science in his *Course on Positive Philosophy* (1830-1842), was well-known in British philosophical circles and his work regarded with a mixture of criticism and approval. 'Sociology,' as such, remained stuck on the written pages of scholars such as William Whewell, John Stuart Mill, John K. Ingram, Herbert Spencer and Frederic Harrison; it was an idea of a social science that only acquired clearer shape and form in Britain in 1903 with the establishment of the first Sociological Society in Britain, which had the Comtean philosophy at its core.

This, of course, does not mean that there was no social science practised in Britain in the nineteenth century. On the contrary, there was a strong interest in both the construction and practice of social science – but there was little agreement on its form and scope (Goldman, 2002). Most prominent were the social reformist movement, best represented by the work of the Social Science Association (SSA) (1857-1886); the statistical movement represented at Section F of the British Association for the Advancement of Science (BAAS), established in 1833 and the Statistical Society of London (SSL), established in 1834; and political economy, which was the only social science represented in academia with the first Chair of Political Economy established at University College London in 1828. As I explain in greater detail later in this chapter, these social science projects differed profoundly from what Comte understood as 'sociology'.

If, then, in nineteenth-century Britain, 'sociology' existed only as a concept, as a yet unrealised idea, shouldn't its history begin with the establishment of its first proper institution? Is the development of social reformism, statistics and political economy, none of which conform to Comte's idea of 'sociology', at all relevant for the establishment of *sociology* in Britain and for its later development throughout the twentieth century as an academic subject? And if so, in what way? Why should the history of these social science movements be examined in its own right and not

merely as a point of reference to what developed as sociology in the twentieth century?

One way of approaching these questions would be to disregard the differences that existed in the nineteenth century between Comte's understanding of sociology and the social science practised in Britain, and assume that the term 'sociology' can be used to describe all sorts of enquiries into social matters. This approach is best exemplified in Kent for whom nineteenth-century British social science is 'empirical sociology' (Kent, 1981) and therefore not only a relevant but an important part of the history of sociology in general.

This approach is convenient for a descriptive account, like Kent's, which gives an overview of what was practised as social enquiry in nineteenth-century Britain. It is, however, an inefficient and potentially misleading approach if we want to examine the dynamics of the relationships between different social science projects, their differences and their particular influence on the future development of academic sociology in Britain.

Few scholars have attempted such examinations. By far the best-known work on this subject is Philip Abrams' 1968 essay on *The Origins of British Sociology (1834-1914).* Abrams pays special attention to the SSA and the SSL, holding them responsible for the failure of British sociology to firmly establish itself both in the nineteenth century and into the twentieth; in his view, 'the failure was not at all an effect of inadequate intellectual resources – it was a problem of institutionalisation' (Abrams 1968: 4). There are two sides to his argument: firstly, the type of social science that was institutionalised in the nineteenth century hampered the early development and institutionalisation of sociology; secondly, the first attempts to institutionalise sociology in the early twentieth century failed and a few years after the establishment of the Sociological Society, the various factions from which it was formed went their separate ways, none having a strong enough programme to ensure their continued existence long term. For Abrams, the history of nineteenth century social science in Britain is relevant and important in the later history of British sociology because this early social science had a *negative* impact on the prospects for a 'successful' early development of academic – theoretically oriented and theoretically sophisticated – sociology.

Since its publication in 1968, Abrams' essay has become a widely referenced sociological 'classic' and, to my knowledge, has not been revised or challenged by a *sociologist*. It is questionable, however, to what extent Abrams' analysis of nineteenth-century statistics and reformism, provides an accurate evaluation of, or an insight into, the role and understanding of science, including social science, in Victorian culture. The historian of social science, Lawrence Goldman (1983, 2002) who has studied this period extensively, focusing particularly on the development of social reformism in the SSA, has challenged Abrams' interpretation that the Association produced a 'role model of the social scientist as a technician of policy' and that as a result its 'influence was critical in frustrating the growth of sociology in the mid-nineteenth century' (Abrams 1968: 44). Goldman shows that Abrams' division between academicism and ameliorism, that what was good for social reform was bad for sociology, is a 'false antithesis' (Goldman, 2002: 316). An important aspect of Victorian culture, Goldman argues, was faith in applying scientific procedures in the construction of a 'science of reform', both as an intellectual discipline and a type of public practice; science and reform were closely intertwined, not only here in Britain, but also internationally. The manifold contributions of Victorian social science can, therefore, be understood and explained through the particular cultural and political context of the time; to dismiss them merely as 'frustrating' the rise of sociology is to impose false expectations on the past and judge Victorian social science on the basis of what it *should have been* rather than of what it was. Abrams' interpretation, particularly his insistence that lack of formulation of general theories was a 'failure' for Victorian social science, shows an inability, or reluctance, to understand the context in which movements such as social reformism took place and a distortion of their historical meaning. For Goldman, therefore, studying the development of social science in nineteenth-century Britain is relevant and important for the development of sociology not because the former frustrated the development of the latter but because sociology had its origins in the frustration of reformism (Goldman, 2002: 344). The social science which Abrams' dismisses as a harmful factor was in fact an important stepping stone, a precursor of academic sociology; without understanding the nineteenth-century society that gave

rise to social science, we cannot fully understand the twentieth century that gave rise to 'sociology'.

By the beginning of the twentieth century, when sociology entered academia, the cultural and political climate had changed and sociology did not bear the central characteristics of reformist social science. But while Goldman's account helps us to understand the cultural and political factors that led to the development of social science along the lines of 'science of reform' in the mid-nineteenth century; and why there was not demand for a reformist-oriented sociology at the beginning of the twentieth century, there are other aspects of the sociology that emerged in Britain at that time that are left unexplained. One such aspect is the non-quantitative orientation of British sociology, not only at the beginning, but throughout all of the twentieth century.

It is this orientation of British academic sociology that has prompted another scholar, John H. Goldthorpe, to re-examine the nineteenth-century history of social science in Britain, Germany and France. Goldthorpe's account is an indirect refutation of Abrams' thesis according to which the development of nineteenth-century statistics is important in the history of academic sociology only as far as it frustrated sociology's earlier development. Goldthorpe sets out to challenge Abrams' implicit view that statistical methods, or even statistical understanding of social phenomena, are somehow 'un-sociological' and therefore unable to make a positive contribution to the development of academic sociology. The 'absent synthesis', as Goldthorpe calls it, between nineteenth-century statistics and sociology is a genuinely surprising situation that deserves examination beyond what appear to be oversimplified judgments about who frustrated the development of whom. Among the intellectual factors that contributed to the separate development of the ideas of statistics and sociology in the nineteenth century were: the dominance of the Comtean legacy, particularly Comte's insistence that sociology, if it were to be a science, should aim at the formulation of deterministic laws and reject any statistical explanations of a probabilistic kind. A second factor was the empiricism of much of contemporary statistical work, which apparently restricted statistics to 'a mindless cult of the facts' (Goldthorpe, 2000: 293). A third factor was the conception shared by many social thinkers, most prominently by Emile Durkheim that the only

appropriate way of explaining 'social facts' was on the basis of other 'social facts' (Goldthorpe, 2000: 293); since statistical explanations were grounded upon the measurement of individual actions, statistics was seen to be unsuitable for sociology.

However, the emergence of a wider statistical worldview and the development of methods for social statistics in the nineteenth century is of much greater historical importance for the development of academic sociology in Britain in the twentieth century than has been acknowledged in the cited scholarship and it is significant for reasons that have either been neglected, not understood (Abrams) or only implicitly acknowledged (Goldman and Goldthorpe). The following chapter aims to explain what these reasons are and to show *why the nineteenth-century 'absent synthesis', to borrow Goldthorpe's phrase, mattered in the nineteenth century and still matters to this day.* Since Abrams' account of the role of nineteenth century statistics in the development of academic sociology has been the most widely read and influential among sociologists in Britain, and since I have found it to be fundamentally flawed, I will pay it particular attention. I will then move on to developing what I believe to be a more accurate picture of the significance of the emergence of statistical methods and of a statistical worldview for social enquiry and the development of social science.

The Importance of Nineteenth-century Statistics in the History of British Sociology According to Abrams (1968)

The history of sociology before 1914, Abrams argued, was 'in no sense a success story' (Abrams, 1968: 4). To understand why, in his view, it is important to examine what was going on in social science before sociology was established institutionally at the beginning of the twentieth century. To Abrams, examining nineteenth-century statistics is particularly important as it had an especially negative impact on the development of sociology in the nineteenth century and later on.

Among the major reasons why the development of statistics in nineteenth-century Britain frustrated the development of sociology is social statisticians' direct engagement with social policy:

> Potential British social scientists […] found themselves in an almost unique situation: government and party politics were open to them, the universities were closed. It is hardly surprising that they succumbed readily to the lure of administrative opportunity. So sociology languished (Abrams 1968: 148-149).

Abrams describes the early statistical tradition as essentially a workforce that was an extension of the British government, lacking in the analytical expertise to deal with the constantly evolving social problems arising out of rapid industrialisation and urbanisation. Organisations such as the SSL, he argues, contributed to the continuous and rapid growth of British government until it became one of the largest of its kind in Europe; they 'reduced the idea of intelligence to a matter of facts and figures' and produced an empirical tradition that led to 'massive but intellectually sterile levers of social reform' (Abrams, 1968: 5). Due to the close proximity between the British government and British social scientists, the 'energies which might have gone toward sociology' were 'soaked up' in 'performing administration and intelligence functions'; as a result, 'the sociologist remained a potential role' (Abrams 1968: 4, 5).

But this was not the sole reason why the existence of a statistical tradition was not conducive to the development of a 'sociology'. Abrams' core criticism of the statistical movement is that they naively pursued empiricist and a-theoretical inquiries, largely devoted to the gathering and evaluation of facts, 'instead of the radical reconstruction of theory [the theories of political economy]' (Abrams 1968: 15). Working with government, he argues, reinforced this empiricism and made 'fundamental speculation' unnecessary (Abrams, 1968: 15) allowing statisticians to cling 'to the atomistic, optimistic perspectives of political economy as their organising frame of reference' (Abrams, 1968: 25); so that, in the end,

> the emphasis settled on the business of producing more exact indicators, better methods of classification and data collection, improved life tables and so on […] The ad hoc administrative orientation remained strong. The commitment was to statistics, not sociology (Abrams 1968: 18).

For an account of 'the origins of British sociology' Abrams' essay is remarkably a-historical – his interpretation lacks in historical detail and in references

to the broader historical context in which the statistical and social reformist movements developed. This is apparent even at the level of terminology – without stating explicitly what he understands as 'sociology', he is clear that in the SSL and the SSA 'the commitment was to statistics not to sociology' (Abrams, 1968: 18). An historical account with a more careful and responsible handling of terminology would have necessarily acknowledged the contemporary meaning of the term 'sociology' and how it developed in the context of the development of statistics and social reformism. As previously discussed, 'sociology' at this time was an idea of a social science largely based on Comte's understanding of the term as shaped by his positive philosophy. It is not uncommon to see the term being used by scholars who did not associate themselves with the Comtean tradition, but it was mainly Comte's followers who used it *consistently*[1]. However, the British Comtean or positivist movement contributed nothing new to Comte's philosophy and failed to develop a sociological theory (Farmer, 1967). It is hard to envisage, therefore, what kind of 'sociology' opportunities Abrams is talking about when he makes the claim that 'sociology' would have taken up these opportunities if it were not for the stifling influence of the statistical and social reformist movements. Speculations of this kind entirely miss the point of understanding the dynamics that were going on between competing visions about what social science could or should be. Instead, Abrams' approach enters the realm of counterfactual history which may be used in preliminary historical reflections but not as a tool to analyse and judge actual historical events – after all, there would be countless possibilities for 'a sociology' in a counterfactual history, making analyses, which employ this technique, meaningless.

It can be inferred from Abrams' account that there is one type of 'sociology' that he regards as proper and which he uses to judge the social science that developed in mid-nineteenth century Britain. In his view, a type of theoretically based and theoretically oriented 'sociology' that can be applied not only to the investigation of society but in the conceptual reconstruction of social phenomena and

[1] Herbert Spencer used the term but would have described himself as a dissenter from Comte's views, particularly with regard to the status of psychology as a science (cf. Spencer, 1864). Although Spencer's work is important in its own right, I do not discuss Spencer's work at length since I have found very little reference, if any at all, to his writings in the works and authors I examine here.

the building of grand theories of society is the only legitimate type of 'sociology'. But where does this concept of 'sociology as theory' come from? Abrams provides no evidence that this is how nineteenth-century scholars thought about 'sociology'. Instead, he appears to be projecting his own 1960s conceptions of 'sociology as theory' (see Part Three) onto the early history of social science – a presentist approach that not only leaves us with a distorted view but provides little insight into the contemporary role and long-term significance of the statistical and social reformist movement. In this respect, at the very least, his account is unsatisfactory and calls for major revision.

Chapter Two

Statistics and Science in Nineteenth-Century Britain

In his monumental work on the history of scientific thought in nineteenth-century Europe, J. T. Merz argued that the nineteenth century might be called 'the statistical century' due to the fact that statistics as a science of large numbers was increasingly being drawn into use in a variety of fields in social and natural scientific research (Merz, 1904: 567). But what was this 'statistics'? Where did it come from and what did it comprise?

It could be argued that the emergence of the statistical (numerical) attitude towards social affairs is to be found in the seventeenth century, particularly in the work of the English political arithmeticians (cf. Lazarsfeld, 1961). The idea that social phenomena could be subjected to quantitative analysis was already present in the work of John Graunt (1620-1674) and Edmund Halley (1656-1742) who published the earliest mortality tables in Britain. A new, *social,* phenomenon was conceptualised in this way – the average mortality rate in a population – to express a new kind of scholarly interest dealing with the laws which govern the occurrence of death in society rather than the reasons why a particular death had occurred. But the aspiration, most clearly formulated by William Petty (1623-1687) that "one had to express oneself in terms of number, weight and measure" (Petty quoted in Lazarsfeld, 1961: 281) indicated not only the rise of new particular type of scientific curiosity but also the emergence of the idea that quantitatively classifying and analysing all social experience increases the chances of better, more efficient, government. And a landmark of eighteenth-century statistical work, J. P Süssmilch's (1707-1767), *The Divine Order as Proven by Birth, Death and Fertility of the Human Species (1741),* provided systematic evidence for the constant ratio between male and female child births in society, initiating the idea that statistical data could provide insight into the unknown, including the previously unknowable, divine providence. This idea later evolved into an argument about the power of statistics to help us reliably predict the future.

The development of the idea that numerical data about society can be used to make sense of societal, not merely individual, behaviour and to assist in a more effective government and prediction of societal matters, ran parallel with the development of a greater understanding of mathematical probability. Throughout the eighteenth century a subjectivist approach to the understanding of probability predominated which was mainly used in the study of individual rationality and decision-making, such as in gambling, risk, insurance policies and criminal law (for further details, cf. Daston, 1987; 1988). But towards the end of the eighteenth and beginning of the nineteenth century there were major changes in the way statistics was practised due to the expanding influence of the frequentist conception of probability. This placed emphasis on the ability to effectively analyse and summarise large amounts of data, thereby enabling us to track the development of large-scale phenomena. Through the study of probability, a rise in interest in inexorable social laws was displacing the study of individual rational action with particular consequences for the conceptualisation of society: whereas in the eighteenth century the study of the reasonable individual or *l'homme éclairé* provided a satisfactory explanation of social affairs (it was believed that society was held together by rational actions and that rational actions could be predicted with the help of the calculation of probabilistic risk) by the early nineteenth century the study of *all individuals, reasonable or not,* was felt necessary. The development of this idea was aided by Poisson's contribution to the understanding of the law of large numbers. He proved that the frequentist approach to probability could be used in the study of social affairs, since the law of large numbers applies not only to events such as coin tossing, where the probability of the individual event is constant, but also to social phenomena where the probability constantly varies (Hacking, 1983a).

But the slow realisation that one can use numerical data gathered from masses of individuals to learn about, govern and predict the course of *society*; together with the evolution of the concept of mathematical probability towards a frequentist approach would not have been enough on their own to give rise to modern-day statistics. Another major component which was missing until the late eighteenth century, was an effective mechanism for the collection of masses of statistical data needed to study society.

We witness a change in government attitudes towards the publication of statistical data from the eighteenth into the nineteenth century. Whereas in the eighteenth century, European states generally kept secret even the little statistics they gathered, often in purely verbal format[2], in the nineteenth century, 'they loved to publish' (Hacking, 1990: 20). The first public attempts at large-scale collection and publishing of statistical data took place in Prussia. It was German thinkers and statesmen, Hacking argues, who 'brought to full consciousness the idea that the nation-state is essentially characterised by its statistics' and as such it 'demands a statistical office in order to define itself and its power' (Hacking, 1990: 18). The realisation of this idea might have originated in Prussia but it very soon spread all over Europe, giving rise to a phenomenon that Hacking has called 'the avalanche of printed numbers', with statistical offices and institutions being set up and given the responsibility to collect and publicise statistical data.

Although the avalanche of printed numbers occurred with almost equal force in all major European countries, not all of them had readers 'of the right kind' to approach the statistical data as a source that could be used for the formulation of societal laws. The Prussians had one of the most powerful statistical bureaus in Europe, but it was the French who used their avalanche of printed numbers to first begin thinking about the statistical laws of society (Hacking, 1990: 35-6), as in, for instance, the work of the Frenchman Marquis de Condorcet (1743-1794). Condorcet is well-known for outlining one of the first detailed conceptions of what he called 'moral science'. It consisted of moral-science-as-history – a branch that dealt with the analysis of overarching structures and the evolution of human society throughout the ages – and of social mathematics. As envisioned by Condorcet, social mathematics was not yet the empirical quantitative study of large-scale social phenomena that we today call statistics; but 'without Condorcet's enlightenment vision of law, of moral science […] those number collecting offices might merely manufactured tables in the Prussian style' (Hacking, 1990: 46).

[2] For further details on the pre-numerical concept of statistics which dominated statistical activity in Germany in the eighteenth century, see Hacking (1990: 24-26); on the introduction of the word statistics into English, see Yule (1905).

Condorcet conceived of the possibility of an empirical quantitative social science in the eighteenth century and the avalanche of printed numbers in the nineteenth century made possible the realisation of this concept and of the concept of statistical laws about society. Nowhere is this more clearly demonstrated than in the work of the Belgian polymath, Adolphe Quetelet (1796-1874). Quetelet developed the idea of 'the average man' and promoted its use explicitly for the purpose of social research (Quetelet, 1842 [1835]). The average man was a way of conceptualising the order and the 'alarming regularity' (Quetelet, 1842 [1835]: vii) that the descriptive analysis of data about social events was revealing. Quetelet shocked the contemporary world by demonstrating that in spite of the irregularity and unpredictability of individual deaths or behaviour, such as the commitment of a criminal offence or getting pregnant, the annual crime, birth and death rates remained stable and *could* be predicted. Based on his research, Quetelet (wrongly) assumed that all social phenomena are normally distributed, i.e. aggregates of statistical data on society form a bell curve. But what is more important than Quetelet's limited assumption about possible distributions of statistical data about society was his realisation that the bell curve itself could be conceived of as an autonomously existing phenomenon; that is, he drew a distinction between the bell curve and the individual data that underlie it. This in turn made it possible to conceive of regular and predictable large-scale social phenomena existing separately but, nonetheless, alongside irregular and unpredictable instances of individual behaviour: the 'statistical paradox – that the most unpredictable single event is predictable on a larger scale – became a social reality[3]' (cf. Panayotova, 2014).

Once the conception and demonstration of large-scale statistical laws was made possible, questions were raised concerning the nature of these laws. At the time when Quetelet was working, the most widely accepted interpretation was that they were a result of the deterministic character of the natural and the social world. As Laplace famously put it, if there was

[3] Many misunderstood Quetelet's argument and accused him of being a materialist, a fatalist and a denier of free will. Many of these accusations, however, were due to critics' hesitation to acknowledge that regular large-scale phenomena do not determine any individual action *per se*.

> an intelligence which could comprehend all the forces by which nature is animated […] for it, nothing would be uncertain and the future, as the past, would be present to its eyes (LaPlace, 1996 [1814]: 4).

Quetelet followed by arguing that 'to one who knew how to foresee all things there would be no chance' (Quetelet, 1849: 9). This idea of uncertainty as resulting purely from human ignorance, and of probability as merely a tool to help deal with that ignorance, remained prevalent but was becoming increasingly weaker throughout the nineteenth century. Hacking (1983a, 1983b, 1990) has described the process as 'the erosion of determinism'; a process which made possible the conception of a world which is inherently stochastic, that is, inherently uncertain and unpredictable. This was first fully explained in philosophical terms by the philosopher C. S. Peirce and demonstrated mathematically by the quantum revolution in physics at the beginning of the twentieth century. A major stepping stone in the *probabilistic* analysis of social phenomena was Francis Galton's discovery of regression to the mean – the mechanism by which variation works, enabling certain average characteristics of the population to remain stable over the course of time – and of statistical correlation, which allowed a probabilistic understanding of the interaction between social phenomena based on how they vary in relation to one another (Galton, 1886; 1888; 1889). Galton's ideas were later developed by Karl Pearson and his team, and together with the introduction of random sampling (Kiar (1897); Bowley, 1906; Kruskall and Mosteller, 1980) led to 'the taming of chance' and a new understanding of statistical laws as 'autonomous'.

This brief overview of the history of statistics from the seventeenth to the early twentieth century shows that the emergence of statistics initiated some major changes in the understanding, analysis, manipulation and prediction of social phenomena and was closely intertwined with it. But how is the history of statistics relevant to the emergence of the idea of a social science, and particularly of sociology, in Britain?

Many sociologists in Britain, and internationally, have acknowledged that a major factor in the rise of the idea of sociology were the societal changes that took place in the Western world, particularly rapid urbanisation and industrialisation. It is rarely acknowledged, however, that part and parcel of the processes of urbanisation

and industrialisation was the aspiration to design more accurate and more efficient ways of gathering information about these processes; and that, crucially, it was *through statistics* that the effects of urbanisation and industrialisation, essential characteristics of the modern world, became *knowable*. Why should this be the case? Older forms of knowing and analysing social phenomena, on the basis, for instance, of general abstract principles of human nature (as we find in Thomas Hobbes, Jean-Jacques Rousseau or John Locke) could be used in societies which are characterised by no or only slowly developing social change; but in a world where social change is rapid, where social phenomena take place on a grander scale and involve aggregates of people, statistics becomes an indispensable form of knowledge because it allows us to get to grips with social change *as it happens* (cf. Gellner, 1988). Without this statistical basis, without this most basic but indispensable knowledge of the state of society and of social change, conceiving of a social science, let alone practising it, would not have been possible in the first place. By the mid-nineteenth century, therefore, the development of statistics had provided a new, workable concept of society – as a 'dynamic entity' that was somehow more fundamental than the state and requiring a social science to comprehend it' (Gigerenzer et al, 1997 [1989]: 39) – and basic methods of the analysis of social phenomena.

The fact that modern society was first and foremost made knowable through statistics is only one reason why it is important to consider the history of statistics when examining the history of sociology as a social science. But there is another, broader and in some ways even more fundamental reason why statistics matters in the history of sociology, especially in Britain.

The changes in social conditions which were created through the urbanisation and industrialisation of western society which, in turn, prompted social scientific concerns, resulted from major scientific discoveries in the fields of physics, chemistry and technology that took place in Britain and other parts of Europe throughout the eighteenth and into the early nineteenth centuries. It was modern science that created the modern world and that created the conditions for the emergence of a society that needs a *social* science to help it to understand and manage itself. But what was science in the nineteenth century? And, crucially, we know that most of statistics up to mid-nineteenth century was about social matters –

but was nineteenth-century statistics also 'scientific'? In other words, did statistics, as it was practised in Britain in the nineteenth century qualify as 'a social science' *according to the contemporary standards of science*? If there is good evidence to suggest a positive answer, then the history of statistics becomes fundamental in the history of sociology, not only because statistics first made possible the study of modern society but because it made possible the *scientific* study of society. If this is the case, and given the advances that science has made possible since the sixteenth century, it appears inconceivable for any social science to disregard statistics. To investigate this question – the core question of this chapter – I first examine the nineteenth-century understanding of science, as outlined in the work of some of the most prominent scientific thinkers in Britain at the time, John Herschel and William Whewell, as well as the philosopher John Stuart Mill. I then compare their understanding to the understanding that the SSL had of their own work. This would then allow us to assess the extent to which the British statistical tradition, as exemplified in the work of the SSL, conformed to the standards of contemporary science.

The Idea of Science in Early Nineteenth-Century Britain

What did it mean to be 'scientific' in early nineteenth-century Britain? This question was at the core of a 1961 study by the historian of nineteenth-century science, W. F. Cannon. On the basis of a detailed study of contemporary debates, comments and major treatises about science, Cannon argues that, in a nutshell, to be scientific meant to be "as much like [John] Herschel as possible" (Cannon, 1961: 218). Furthermore:

> In England of the 1830s, "to be scientific" meant "to be like physical astronomy". To be quite specific, it meant to be like John Herschel's extension of physical astronomy […] (Cannon, 1961: 238).

It was not without good reason that Cannon chose John Herschel as the epitome of science in 1830s Britain. John Herschel was the son of the famous

astronomer William Herschel, who refined astronomical methods for observation, designed new and much more powerful telescopes than were previously available and in 1781 discovered the seventh planet in the Solar System, Uranus. In some respects, however, John Herschel managed to surpass his father's extraordinary achievements – he quickly developed into a polymath, with contributions to physics, astronomy, pure mathematics, chemistry, galvanism, photography and, last but not least, philosophy of science. He was not only famous; he was admired by many: from John Ruskin to Richard Owen; from Charles Darwin to John Stuart Mill and was at the centre of a circle of scientists, including William Whewell, Charles Babbage, Adam Sedgwick and Adolphe Quetelet all of whom were in their own way revolutionising science and the way people thought about it. Herschel's work on the philosophy of science – *Preliminary Discourse on the Study of Natural Philosophy* (1851, [1830]) had a major impact on the understanding of science and it is on this work that I rely for clarifying what was understood by science in the early nineteenth century.

For Herschel, and for the young generation of scientists who were active in the 1830s, an important aspect of science was its public and popular character – scientific achievements had to be communicated between scientists but also to ordinary people; and it was scientific achievements – 'not the experience of one man only, or of one generation, but the accumulated experience of all mankind in all ages' – that should be used in judging the credibility and advance of science (Herschel, 1851 [1830]: 76)[4]. But openness and broad communication of science was becoming increasingly difficult within the old English scientific circles. The Royal Society, the alleged scientific authority in the country, had been in decline since the eighteenth century, mainly due to the exclusive character of its membership and the fact that it had fallen under the influence of aristocratic dilettantes instead of major contributors to scientific knowledge (Basalla et al, 1970). The appointment of the Royal Duke of Sussex as a President of the Society instead of John Herschel in 1830 was yet another sign that the Society could not offer a good platform for the development of the kind of science that the wider scientific community and the general public wanted to see. It was the dismissal of Herschel that triggered a crisis in the Royal

[4] For a detailed overview of the emergence of the idea of scientific progress through co-operation for non-personal ends, see Zilsel (1945).

Society and that made necessary the establishment of another scientific institution that could fulfil the expectations of the new generation of scientists.

The British Association for the Advancement of Science (BAAS) was established in 1831 with William Vernon Harcourt as a chairman of the management committee with this idea in mind and

> having for its objects, to give a stronger impulse, and more systematic direction to the objects of science, and a removal of those disadvantages which impede its progress, and to promote the intercourse of the cultivators of science with one another, and with foreign philosophers (Hartcourt 1831, quoted in Orange, 1981: 43).

Among its founding members were Herschel, Babbage, Whewell, Sir David Brewster and J. F. W. Johnson. Unlike the Royal Society, which was solely London-based, the BAAS was established as a peripatetic, or travelling, organisation – its annual meetings took place in different cities each year and in the 1830s it visited many of the industrial cities where public unrest was a serious issue. Again, unlike the Royal Society, the BAAS was based on a more or less democratic principle and was open to laypeople – the technical papers were presented at the sectional meetings, whereas the presidential addresses and the public lectures were directed to everyone, including the working classes (Basalla et al, 1970). Membership was not restricted to any particular political or religious affiliation. And finally, unlike other travelling political, social and intellectual coalitions that provided opportunities to unify members of the 'better' classes by focusing on morals, religion and education, the BAAS chose science – abstract and universal, non-political and non-sectarian knowledge – as a way to foster vertical unification among the classes (Thackray and Morrell, 1981). Overall, the establishment of BAAS was a manifestation of a wider movement characterised by a vast increase in scientific activity (the number of scientific societies and clubs, scientific chairs and journals had increased rapidly from the beginning of the nineteenth century onwards, cf. Thackray and Morrell (1981)) and a strong desire to communicate science to wider audiences. Nineteenth-century science was not 'technical' like twentieth- or twenty-first century science – instead, it was associated with the ideal of public reason; the purpose of science was

not merely to bring about scientific progress but also to help to sustain the moral, economic and intellectual order (Pearson, 1892; Porter, 2009).

There were other aspects of science that were being redefined in the 1830s. One of them was the term that was used to describe scientific activity. Prior to the 1830s there were a variety of terms used to describe scientific work and the people involved with scientific work, the most popular being natural philosophy/natural philosopher. However, as William Whewell pointed out, a new term was necessary due the 'increasing proclivity of separation and dismemberment' of scientific pursuits ([Whewell], 1834: 59). In 1834, Whewell coined what he thought was a more accurate and more appropriate term to replace the ambiguous 'natural philosophy' as a 'general term by which these gentlemen [of science] could describe themselves with reference to their pursuits' – science ([Whewell], 1834: 59).

Although the change of terms did not involve a change in content (the majority of scientists in the 1830s were, in practice, involved in the same kind of work as natural philosophers before them), unlike natural philosophy, science emphasised particular elements in the enquiry into the natural world. By the 1850s, 'science' was exclusively confined to describing the inductive, experimental and physical branches of knowledge (Ross, 1962); for the first time, special attention was being given to 'science as method'– a principle, supported in the BAAS, that science is not 'matters known' but a special mode of knowing (Goldman, 2002: 314; Yeo, 1993).

This was made particularly clear in Herschel's *Preliminary Discourse.* Herschel argued that 'the great and indeed only ultimate source of knowledge of nature and its laws [is] experience' by which he meant not only the experience of individual scientist but the cumulative experience of generations of scientists in studying nature through scientific experimentation and observation, constantly revising and improving knowledge (Herschel, 1851 [1830]: 76). Both observation and experiments necessitate the collection of data, or facts which form the basis of all scientific enquiry:

> Whenever, therefore, we would either analyse a phenomenon into simpler ones, or ascertain what is the course or law of nature under any proposed general contingency, the first step is to accumulate a sufficient quantity of well ascertained facts or recorded instances bearing on the point of question (Herschel, 1851 [1830]: 118).

However, Herschel was clear that facts alone were not what science was about – science advances not only through better and wider access to relevant data but also through a better and more accurate way to describe or interpret these data. He argued 'it is principles, not phenomena – the interpretation, not the mere knowledge of facts – which are the objects of enquiry' (Herschel, 1851 [1830]: 13). A further elaboration of this idea comes from another major contemporary scientist, William Whewell, who was a good friend of Herschel and like Herschel was a faithful admirer of Francis Bacon. Whewell wrote a treatise on the philosophy of science called *The Philosophy of the Inductive Sciences Founded upon Their History* (1847 [1840]) in which he attempted to outline an application of the plan of Bacon's *Novum Organum* to the present condition of physical science. At the core of Whewell's argument was the idea that all scientific knowledge is based on experience *even though* 'experience is unable to prove a fact to be universal' (Whewell, 1847 [1840]: 62-4). Whewell's understanding of science as an activity that proceeds on the basis of hypothesis formulation and testing and that we can reject or accept hypothesis but never prove them, was later revised by Karl Popper, and is still the predominant logic used in science today (for a more detailed overview of Whewell's views, see Buchdahl, 1971).

Putting experience and collection of data at the centre of science makes it necessary to pay special attention to measurement**.** This was another definitive characteristic of science that was made explicit in Herschel's and Whewell's work. Herschel argued that:

> […] it is not merely in preserving us from exaggerated impressions that numerical precision is desirable. It is the very soul of science; and its attainment affords the only criterion, or at least the best, of the truth of theories, and the correctness of experiments (Herschel, 1851 [1830]: 122).

Later, Whewell adopted and emphasised Herschel's point that quantification is essential for the formulation of scientific laws and argued that the laws of greatest scientific value were the laws expressed in quantitative terms (Whewell, 1847 [1840]; Ruse, 1975).

This takes us to another definitive characteristic of Victorian science – commitment to the discovery of laws. Herschel argued that scientists had come to understand that there was 'little prospect' that 'in our investigation of nature we shall ever be able to arrive at a knowledge of ultimate causes' and that scientific enquiry is limited to the discovery of laws (Herschel, 1851 [1830]: 87-8). Herschel distinguished between 'empirical laws' – those 'derived by the direct process of including in mathematical formulae the results of a greater or less number of measurements' (Herschel, 1851 [1830]: 178) and universal laws – those which have been verified by a deductive process and examination of how nearly they represent the observed facts.

For the process of measurement and law formulation to work properly it was a necessary prerequisite that science is free from prejudices or values and kept separate from political speculation. Herschel distinguished between prejudices of sense (e.g. optical illusions) and prejudices of opinion. The latter was seen as especially damaging to science and Herschel recommended that:

> […] before experience itself can be used with advantage, there is one preliminary step to make which depends wholly on ourselves: it is the absolute dismissal and clearing the mind of all prejudice, from whatever source arising, and the determination to stand and fall by the result of a direct appeal to facts in the first instance (Herschel, 1851 [1830]: 80).

The exclusion of 'prejudices of opinion', however, did not mean that scientists could not, or should not speculate or theorise about their findings. Theory formation was considered an important step in the formulation of scientific laws that can even form a distinct branch within each scientific subject. It is normal, Herschel argued, that in those sciences which have achieved 'a higher degree of consistency and generality' the province of 'the observer could be separated from that of 'the theorist' (Herschel, 1851 [1830]: 131). Moreover, using early Victorian geology as an example, Herschel emphasised that not only can observation be done separately from theorising but that the former can precede the latter – that is, there was nothing unscientific in geologists putting their efforts into collecting fossils and other kinds of data *before* they proceed with the formation of geological theories (Herschel, 1851 [1830]: 131).

Last, but not least, it was understood that the application of the scientific method as described above was not limited to the realm of the natural world. The same principles which were being used to investigate nature could be used to investigate the social world:

> The successful results of our experiments and reasonings in natural philosophy, and the incalculable advantages which experience, systematically consulted and dispassionately reasoned on, has conferred in matters purely physical, tend of necessity to impress something of the well weighted and progressive character of science on *the more complicated conduct of our social and moral relations*. It is thus that legislation and politics become gradually regarded as experimental sciences […] (Herschel, 1851 [1830]: 72-73).

Although he did not use the term 'social science' (this term was first introduced into English by John Stuart Mill in 1836, cf. Senn, 1958), it is clear from Herschel's statement that the application of the scientific method to social matters was not only thought possible, but *desirable*. Herschel was well acquainted with contemporary developments in statistics and probability – in his *Preliminary Discourse* he discussed in detail the advantages of the method of averaging; he wrote a detailed review of Quetelet's work (Herschel, 1850) from which it was clear that Herschel was 'deeply impressed by statistical regularities which indicated the possibility of a science of man governing aggregates without constraining individuals' (Porter, 1986: 120).

Herschel's personal support for the potential of Quetelet's statistical method in facilitating the scientific investigation of the social world, carried much weight in 1830s Britain; but this would not be enough in itself to prove that statistics fitted the general conception and aspirations of science in this period. It is also necessary to examine the way British statisticians themselves thought about their work and about science and to check whether the views held more widely in institutions such as the SSL corresponded with the views of prominent scientists such as Herschel. If this is the case, then we have evidence that the development of statistics was part and parcel of the development of modern science and as such holds an important role in the history of any social science, including British sociology.

Chapter Three

The Statistical Work of the SSL and Section F of the BAAS: Between the Hammer of Politics and the Anvil of Science

All the characteristics of science that were mentioned above – open and public character; basing knowledge on experience; measurement; formulation of laws; value freedom; acceptable division of labour between theoreticians and observers; application of the scientific method to all areas of human knowledge, including knowledge about society – are compatible in principle. However, when in the 1830s scientific thinking turned towards the realm of moral or social studies, it became clear that maintaining that the scientific method could be successfully applied to the social realm while at the same time making sure that social science is value-free, presented a significant challenge. Of all the characteristics of science, as it was understood in the 1830s, value freedom and freedom from politics was crucial when it came to the establishment of *social* science since, as contemporaries understood it, if social science was at all possible, it had to be value-free; otherwise, it would not be science. The following section follows the debates on this issue that took place in the BAAS and led to establishment of a social science Section in the BAAS with a view of showing that while keeping social science free from politics posed a significant challenge, hindsight allows us to reconsider our understanding of that challenge and see it in a different way from the way contemporaries, and even twentieth-century scholars, used to see it.

The 1830s was a decade of intense political upheavals in Britain. Public support for parliamentary reform had never been greater and some major, even though not very effective, political reforms were carried through. The 1832 Reform Act almost doubled the electorate in England and Wales, extending it to some members of the middle classes and enabling nearly 18% of the adult-male population of England and Wales to vote. The vast majority of the working classes and women, however, were still disenfranchised and the Act brought little change to their lives. The Poor Law Amendment Act in 1834 aimed at fundamental changes in the poor relief system in Britain. Although the Act attempted to reduce the cost of poor relief

and improve the condition of the very poor by building workhouses, it is controversial to what extent it succeeded in bringing about positive change in the lives of the very poor. There were more than a hundred Royal Commissions supervised by Edwin Chadwick in this period (1832-1846). At the same time, through public protest and petition, the Chartist movement was fighting for the rights of the working classes and against the harsh working and living conditions brought about by the industrial revolution (Thackray and Morrell, 1981: 17). The Tory desire to preserve ancient privilege and the status quo and the Whig concern for limited stabilising change clashed dramatically during this period. Although they did not lead to any 'revolutionary' changes, parliamentary change had begun, causing enormous political tension among the classes. Politics had never been such a dividing issue for those who thought that a study of society is not only possible but desirable, and the question of how to produce unbiased, value-free knowledge of society with a firm basis in scientific facts was becoming ever more prominent.

The rapid social change and upheaval in the 1830s created a fresh and more urgent need for a better, more 'scientific' understanding of *society*: interest in the social condition of England among the educated classes grew stronger and an institution like the BAAS, which was established at the beginning of the 1830s with the idea of bringing about progress in all human knowledge, could not prevent social issues being brought before them. But the intense social and political climate also posed great challenges to the *production* of scientific knowledge – the BAAS knew that if they set up 'a social science' division, they had to put emphasis on 'science' and that this, before all, meant making sure that 'social science' remains value-free and separate from politics. Through the work of William Whewell in particular, the BAAS had forged a consensus about natural science's place in culture (Yeo, 1993: 31); now the challenge was – could the same be done for social science? Could BAAS help social science establish a place in British culture from which to enhance knowledge about society whilst avoiding the promotion of particular political ideologies or doctrines?

Given how the only existing branch of knowledge at the time that dealt with social questions, political economy, had developed to date, the prospects of success looked slim. In the 1830s, political economy was very much under the influence of

David Ricardo and based its knowledge on deductive methods and speculation; empirical evidence was considered occasionally, as, for instance, in Adam Smith's *The Wealth of Nations* (1776) or Thomas Malthus' *Essay on the Principle of Population* (1798), but the evidence was neither collected, nor analysed systematically. Within the circles of the BAAS, Ricardian political economy was not seen as a 'proper' science – Whewell and Babbage disagreed with the deductive method of Ricardian political economy, which was based on abstract principles about human nature, not careful examination of empirical evidence; and Babbage, for instance, commented that political economists should be reproached for 'too small a use of facts and too large an employment of theory" (cf. Goldman, 1983: 598). Within the circles of the BAAS, the state of political economy exemplified, very clearly, not only the dangers of a potential 'social science' but also the need to reform it. But how?

The avalanche of printed numbers across Europe provided a novel opportunity – could these data be used to study society *scientifically*? The idea of using statistical data to study society was becoming increasingly attractive since it appeared that statistical data held the potential of answering two types of question that abstract political economy could not answer. Firstly, questions about 'What is happening *now*?'; and, secondly, questions about 'How what is happening now is different from what was happening in the past, or may be different from what will happen in the future?' The modern industrial, urbanised world was a world of change, in which the grand theories of human nature or the theories based on absolute conditions that were the foundations of contemporary political economy, were becoming increasingly inept *on their own*. It was thought that statistics could help those interested in a scientific study of society go beyond political economy, substitute its methods with empirically based methods and thereby grasp social change.

Some prominent men of science, who were attending the BAAS annual meeting in Cambridge in 1833 – Thomas Malthus, Adolphe Quetelet, Richard Jones, Charles Babbage, Colonel Sykes and John Elliot Drinkwater – thought that this could be done and that statistical data could help them not only establish an inductive social science based on empirical evidence but reform political economy into inductive economics (cf. Goldman, 1983). The way they envisaged this could be

done was to establish a statistical section within the BAAS. According to Babbage's own account, the idea of the necessity of a statistical section in the BAAS came to him at a private meeting with Jones and Quetelet. The latter had come specially to attend the third meeting of the BAAS 'with the most valuable budget of statistical information' (Babbage, 1961 [1853]), for which, Babbage reckoned, there was no place in any of the Sections. When Babbage and Jones discussed the idea of the establishment of a statistical section, they both agreed that 'unless some *unusual* course were taken, it would be impossible to get such a Section organised until the meeting of the following year' (Babbage, 1969 [1864]: 433). Babbage and Jones decided, therefore, that it was necessary to present the establishment of a statistical section to the General committee as a *fait accompli* and attempt to convince them of 'the great advantage to the British Association of rendering such a section a permanent branch of its institution' (Babbage, 1969 [1864]: 434). Section F, as it came to be known, was thus established informally in 1833 – only afterwards was it approved officially by the BAAS's general committee.

The group who agreed to the establishment of Section F were individuals of high scientific and social reputation whose prestige and influence played a part in the decision of the BAAS to go ahead with the formal establishment of a statistical section. However, as Cullen (1975: 77-90) points out, Babbage, Drinkwater, Sykes, and Jones were all known to hold Whig political views and were all in one way or another involved in politics. At a time when an important characteristic of science within the circles of the BAAS was its separation from politics, the involvement in politics of Babbage and the men in his circle, would have triggered great suspicion in the BAAS regarding their intentions. It is therefore, more reasonable to assume, that the BAAS agreed to a Section F due to an increasing general pressure to engage with social issues scientifically, and that what Babbage and his circle did was to give the final push toward the realisation of this idea.

The BAAS agreed to establish a Section F on the condition that statisticians restrict their work to collection and tabulation of data and do not engage in political opinions about data. However, the official statements of the scientists in charge of BAAS at the time indicate that there was a lack of sufficient clarity regarding the difference between the interpretation of statistical facts in terms of more general

theoretical principles; and the expression of support for political doctrines on the basis of statistical facts. Adam Sedgwick's presidential address to the BAAS in 1833 shows this clearly. On the one hand, Sedgwick argued that:

> […] if we transgress our proper boundaries, go into provinces not belonging to us, and open a door of communication to the dreary world of politics, that instant will the foul Daemon of discord find his way into our Eden of philosophy (Sedgwick, 1833 quoted in Cullen, 1975: 79).

On the other hand, he also insisted that facts are of little value unless 'combined together so as to lead to some philosophical inference' (Sedgwick, 1833, cited in Thackray and Morrell, 1981: 292). The 1833 report of the BAAS also seems to suggest that the formulation of general laws falls within the province of the statistical work of Section F, but that statisticians had to restrict themselves to studying

> those classes of facts relating to communities of men which are capable of being expressed by numbers, and which promise, when sufficiently multiplied, to indicate general laws (BAAS, 1833 quoted in Thackray and Morrell, 1981: 292).

It is uncertain, however, that if the BAAS had spelled out more precisely the difference between theory and political bias, this would have prevented social statisticians in Section F from getting involved in political issues. The grave fears expressed by BAAS in 1833 quickly found justification in the work of the two major statistical organisations that came into existence in 1833 and 1834 – the Manchester Statistical Society and the Statistical Society of London (SSL). The latter had sprung into existence from Section F itself, with the majority of its founding members also being involved in Section F. Some of the most significant papers presented in the first few years of the Section's existence, mainly by members of the London and Manchester Statistical societies, were politically engaged – prominent examples include Cleland's 1834 paper discussing the operation of the Poor Law; Langton's 1835 and 1836 papers attacking the current state of education and Parliamentary returns; the 1840 debate between Alison and Chalmers on pauperism (Thackray and Morrell, 1981). The limited success of establishing a politics-neutral social science

also led to a quick disillusionment with social *science* among those who, at the beginning of the 1830s, were optimistic about the possibility of developing an inductive approach for social science and reforming political economy (cf. Goldman, 1983). In 1837, William Whewell declared that "the statistical section ought never to have been admitted into the Association"; and in 1840, with regard to Chalmers' paper on the destitution of the poor in Scotland, Whewell commented that it "involved exactly what it was most necessary and most desired to exclude from our proceedings" (Whewell quoted in Thackray and Morrell, 1981: 294). It does not appear, however, that Whewell provided much evidence to back up his critique of Chalmers' paper and the extent to which Chalmers faithfully represented or deviated from his empirical data, indicating that, as he was getting involved in other projects, Whewell was perhaps losing his overall enthusiasm for working to improve social science. He was not alone – as Goldman (1983) explains, as the 1830s rolled on, Section F's founding members directed their efforts elsewhere (Malthus passed away in 1834, Quetelet was in Brussels, Whewell and Jones took up various other intellectual activities and by 1839 both Babbage and Jones had left the SSL Council).

The BAAS, however, could not so easily ignore what was happening in Section F and took immediate steps to redress the dangerous situation created by the publication of politically charged papers. The representation of the London and Manchester Statistical societies in the BAAS General committee was limited to only three delegates from each Society; cities of great social destitution, such as Glasgow, were to be avoided in the future; and a 'firm president who understands our objects and will not permit his section to deviate from their straight path of numbers' had to be installed in 1841 – Colonel Sykes, known as 'a strict disciplinarian who denounced speculations unsupported by facts' was perceived as the right man for the job (Thackray and Morrell, 1981: 296). Despite the early attempts of the founding members of Section F, and of BAAS more generally, to define the boundaries of the social science in the Section, as well as in the newly established statistical societies, in such a way as to conform with the boundaries of science, they appear to have underestimated the challenges they were facing.

In the following decades, the importance of keeping social science strictly separate from politics remained a major preoccupation within the statistical Section F.

A decision was made in 1856 to change the name of the Section from 'Statistics' to 'Economic Science and Statistics', which may be interpreted as a sign of growing confidence among members that political biases can, after all, be overcome in the name of an economic *science*. But presidential addresses in fact indicate that in the next few decades, the Section was still struggling to meet its own expectations.

In 1856, Section F's president, Lord Stanley, devoted almost his entire address to explaining the characteristics, procedures and aims of statistics. Stanley did not refer to economic science as, he explained, he had prepared his address prior to the meeting at which the decision was made to incorporate economic science into the Section. However, even the remarks he made with regard to statistics sounded like yet another plea to tread carefully when applying the results of statistical analysis to social questions:

> [statistics] proceeds wholly by the accumulation and comparison of registered facts […] from these facts alone, properly classified, it seeks to deduce general principles […] it rejects all *a priori* reasoning, employing hypothesis, if at all, only in a tentative manner, and subject to future verification (Stanley, 1856: 306).

It is important to interpret a statement like this carefully – the idea was that one kind of thinking, the *a priori* abstract reasoning of the early nineteenth-century political economy, can be substituted for another one in which all conclusions derive from careful observation and are further verified by observation. The idea of *testing a priori* hypothesis was not fully formed yet.

The dangers of mixing political bias and *a priori* assumptions within social science were the focus in the 1860 presidential address by Nassau Senior, at the beginning of which he explained that he had

> looked through the papers which since that time [1856] have been communicated to us, and I have been struck by the unscientific character of many of them. I use that word not dyslogistically, but merely distinctively, merely as expressing that the writers have wandered from the domain of science into that of art (Senior, 1860: 357).

Senior's address does read a little like a guidance manual, in which he outlines as carefully as possible what a political economist is permitted and not permitted to do. He sums it all up as follows:

> The subject matter of Political Economy is, I repeat, *wealth.* […] Whenever he gives a precept, whenever he advises his reader to do anything, or to abstain from doing anything, he wanders from science into Art, generally into the art of morality, or the art of government (Senior, 1860: 359).

In 1864, another presidential address, by William Farr, began again emphasising that statistics is a science 'of the relations of numbers of men' (Farr, 1864: 459). But Farr also gave examples of the ways in which, when combined, economics and statistics can produce adequate practical as well as scientific results – consideration of statistical data, for instance, had pushed countries towards free trade by showing that protective tariffs harm the economy; and the consideration of vital statistics have helped to improve the health and moral condition of the people (Farr, 1864: 472).

However, achievements like the ones discussed by Farr did not put a stop to the warnings issued with regard to the unscientific use of statistics. The President for 1876, George Campbell, began his address by reminding the Section that its object was:

> to follow as far as may be a strictly scientific method of enquiry, not lapsing in the discussion of political details but attempting to ascertain the principles on which economics results are founded, and to define the main lines of economic truth (Campbell, 1876: 648).

But neither the reported achievements, nor the presidential warnings prevented the issue from continuing to escalate and in 1877 a crisis loomed in the circles of Section F. The crisis began with deliberations within the BAAS council over whether Section F did or did not meet the scientific standards of the Association. Two reports were laid in front of the council, one for and one against the abolition of the section, by Francis Galton and William Farr, respectively.

Galton criticised Section F for allowing papers of eclectic and unscientific character to be read before the Section. He provided a list of the papers presented

between 1873 and 1875, arguing that none of them dealt with the mathematical theory of statistics and that very few represented what science, in the strictest sense, was meant to be – 'precise measurement' and 'definite laws' (Galton, 1877a: 471). Galton did not analyse the papers he condemned as 'unscientific' so it is not absolutely clear if his objections were based purely on the subject matter of the papers (economic law of strikes, national debt, Indian railways, education, drunkenness, sugar trade, domestic service of gentlewomen, the cost and propriety of removing to England the Fallen Obelisk of Alexandria) or also on the method of investigation employed in the study of these subjects. His verdict, however, on the choice of subject matter was a condemnation: 'few of the subjects treated of, fall within the meaning of the word scientific'; moreover, 'the few of them that do, would be wholly insufficient to occupy the time of the Section' (Galton, 1877a: 471).

Alongside Galton's paper, in which he advocated the closing down of the Section, the BAAS General Committee received another paper signed by William Farr and the two Secretaries of the SSL, who advocated the continuation and reformation of the Section. They admitted, as Galton had argued, that:

> Many unscientific papers have been brought before Section F and these have been reported and discussed to the neglect of really scientific papers on other subjects […] Its subjects are also those of practical politics and philanthropy, and this brings to the discussion people who are politicians and philanthropists but who are not men of science (Farr, 1877: 475).

Unlike Galton, however, they did not think this was a good enough reason to close down the section; instead, they suggested that Section F devises and applies rigorously most stringent rules to prevent the admission of unscientific papers.

Both Galton and Farr, therefore, agreed on the nature of the problem, but provided different solutions. Section F continued to exist but the clear and present danger of political bias continued to cast a long shadow over its work – a few years after the crisis was resolved, in 1881, the President, M. E. Grant Duff, insisted that it was 'imperatively necessary' to 'refuse a hearing to all who wish to discuss burning questions of English politics even although they have a scientific side' (Duff, 1881: 659). The strong presence of such concerns displays a sort of an obsession among the statisticians and the economists in Section F to solve the problems that could result

from allowing political bias to influence statistical analyses; but it also indicates that they were aware themselves that progress in this respect was difficult and that they had to make continuous efforts to achieve it.

What do we learn about social statistics from the debates in Section F? And what significance do they have for our understanding of the conditions in which British sociology later emerged? Our knowledge about the debates in Section F derives primarily from three major studies on this topic – Thackray and Morell (1981), Cullen (1975) and Goldman (1983) – on which I have partially relied to explain the establishment of Section F and the debates about the value-freedom of social science more generally and social statistics in particular. These historical accounts have a particular focus on the kind of values or political bias that was interwoven in the statisticians' or economists' science and aim to show that science has never been and could never be 'pure' or clear from political or any other kind of bias that arises from the social conditions in which science is being done. They aim to portray science as a social process governed by social factors that affect the decisions taken by scientists and their scientific work. Cullen for instance, shows clearly that nearly all of the founding members of Section F held Whig political views, which would mean that they could use statistics to further the political aims that were associated with the Whig party. The importance of these factors should be acknowledged and Thackray and Morell's and Cullen's accounts have made a valuable contribution in this direction.

However, such accounts tend to obscure other important elements of the historical significance of the work with which Section F was engaged. We need to ask ourselves – did the Whig views of Babbage and Whewell make them lesser scientists? Did these views interfere with their plans and aspirations to develop the scientific method and promote its application in the study of society? Thackray and Morell, and Cullen show no evidence that this has been the case. Over a hundred years later, we are still making efforts to separate values from facts – but that of course does not mean that statistical methodology has not advanced. At the time of Section F, just as now, there were people misusing statistics and people who handled them fairly and skilfully; and the ability to tell when statistics have been misused has constituted a great deal of the methodological advancement over the years. It is

important to acknowledge that the fact that Section F discussed values in the first place and acknowledged the danger of mixing values and facts is just as important as the existence of the danger itself. That the founding members of Section F were Whigs did not make them lesser scientists; it would have been unscientific if they did not recognise the danger of letting political values influence their judgment.

Another important point that has been missed by historical accounts focusing on the social and political influences on the scientific work of the BAAS is what made the debates about value freedom possible in the first place? What led Babbage, Whewell, Malthus, Jones and Drinkwater to believe that a reformation of political economy into inductive economics was possible in the first place? We have seen that for them a social science could only be a *science* if it steered clear of political issues and controversies; but one subtle, but crucial, element that both Babbage and his circle, and Thackray and Morell and Cullen in their analyses, ignore is that the distinction between facts and values would not have been possible *if statistical data about social phenomena were not available*. Because the scholarship on the establishment of Section F has focused on the limited success of the statisticians' attempts to distinguish between value-free and politically charged social science and thereby reform political economy, they have ignored the fact that in the 1830s such attempts were possible *for the first time ever*. One could have political economy *before and after* the avalanche of printed numbers; but one could have inductive economics only *after* statistical data had swept Europe. This was clearly grasped by Quetelet:

> Hitherto, the science of man has been limited to researches, more or less complete, respecting some of its laws, to results deduced from single or insulated observations, and to theories often based on mere glimpses […] (Quetelet, 1842 [1835]: 5).

It was only in the early nineteenth century that this situation could be overcome; that another way of studying social questions became possible. As William Farr remarked in 1864, 'it is only in civilised communities and in recent times, that it [statistics] finds adequate materials' (Farr, 1864: 459). Before the avalanche, there were only 'values'; after it, there were both 'facts' and 'values'. What this meant was that, regardless of how successful Section F or the SSL were in

keeping values and facts separate, they were now able to produce genuinely new knowledge about the social world that was not possible before. We can see this now only in hindsight but, in cases like these, hindsight proves extremely valuable as it allows us to get a glimpse of the big changes that even the scientists of the day failed to fully recognise. It is this, rather than the awkward attempts of Section F to establish a value-free social science, that allows us to see why the rise of statistics is important in the course of historical events that eventually led to establishment of British sociology.

Chapter Four

The Legacy of the Statistical Society of London – *Aliis Exterendum* and Nothing Else?

As I explained above, in order to understand the impact of science on the development of the study of society, it is necessary to modify previous historical accounts by putting the work of Section F in the context of just what statistics had made possible – the arrival of facts changed knowledge of the social world in a long-term and fundamental way that no political doctrine alone could ever have done.

However, while Thackray and Morell, and Cullen emphasise the social and political context in which the work of the BAAS took place, scholars like Abrams (1968) have held the 'focus on facts' and the 'absence of theory' in the work of the Statistical Society of London as having been responsible for frustrating the development of sociology. But it is important to consider carefully whether Abrams' focus on the SSL's collection of statistical data did not prevent him from seeing some other important details of the SSL's work (just as Thackray and Morell's and Cullen's focus on the particular political biases of the Section F members prevented them from seeing how novel the opportunities available to Section F actually were). In the following section, I examine the major developments in the SSL in the light of the nineteenth century idea of science, aiming to show that the SSL's attempts to produce value-free work did not necessarily mean that they did not recognise the importance of theory in their work.

The Statistical Society of London was established in March 1834, less than a year after Section F. According to Charles Babbage's account:

> The interest of our discussion, and the mass of materials which now began to open upon our view, naturally indicated the necessity of forming a more permanent society for their collection. The British Association approved of the appointment of a permanent committee of this section. I was requested to act as a Chairman, and Mr. Drinkwater as secretary. On the 15th March, 1834, at a public meeting held in London, the Marquis of Lansdowne in the Chair, it was resolved to establish the Statistical Society of London (Babbage, 1961 [1864]: 434).

The members of the newly established society and those of Section F were nearly identical (for a detailed list, cf. Rosenbaum, 1984). And, perhaps not surprisingly, the Society was facing problems which were similar to the problems that Section F faced, particularly with regard to its position on science (facts) and politics (values): the problem was how to produce politically unbiased scientific work which was nonetheless socially relevant? The strategy that the Society employed to cope with this difficulty has attracted a lot of attention and is the most widely discussed issue from the Society's early years (Abrams, 1968; Cullen, 1975; Hilts, 1978; Unknown Author, 1838). A full account of the events surrounding the decisions of the Society during the 1830s is available in these sources, particularly Hilts (1978), and it is not necessary to go into detail here. But a brief outline *is* necessary in order to make a couple of important points about the work of the Society with the aim of showing that Abrams' 1968 account, which has been the most influential account in *sociological* audiences, requires modification.

When the Society was established, it had to consider the condition under which Section F was accepted into the BAAS – that the Section will restrict its work to numerical facts and provide 'the raw material for political economy' (Sedgwick quoted in Hilts, 1978: 34) and avoid engaging in political issues. Although such restrictions were hardly compatible with the views of the Society's members, many of whom were political economists, politicians and social reformers (Hilts, 1978: 36), the Society had to design some strategy that would help them monitor the work of its members and also convince the public that the work they were doing was not a product of political bias, but had a strong foundation in the 'facts'.

The strategy which was expressed clearly in the 1834 prospectus of the Society was that the Society 'will consider it to be the first and most essential rule of its conduct to exclude carefully all Opinions from its transactions and publications' and 'to confine its attention rigorously to facts' (Statistical Society of London, 1834 quoted in Bonar and Macrosty, 1934: 22). Later on, it was made clear how this would square with the fact that most of the Society's members were political economists:

> The Science of Statistics differs from Political Economy, because although it has the same end in view, it does not discuss causes, nor reason about

> probable effects; it seeks only to collect, arrange and compare, that class of facts which alone can form the basis of correct conclusions with respect to social and political government (Statistical Society of London, 1838: 1).

This strategy was exemplified under the name of *aliis exterendum,* meaning 'to be threshed out by others', which was the motto of the Society in the period between 1838, when the *Journal of the Statistical Society of London* was founded and 1856, when the motto was officially removed.

It is this strategy, together with the closer affiliation of the members of the Society with the British government, that, according to Abrams, had a crucial role to play in perpetuating, what was for him, an essentially un-sociological tradition of enquiry – the SSL restricted itself to the collection and tabulation of statistical data and abstained from speculation. Since it grew to become the most influential civil society institution devoted to the study of society in nineteenth-century Britain, it suppressed any opportunity for other, more theoretical approaches, to develop.

One important aspect that Abrams neglects but which is clearly recognised by Hilts is that, for all its shortcomings, the strategy of *aliis exterendum* 'had its adaptive value' by helping the Society meet the needs of the time: to deflect the accusation that its work was excessively political and to assert 'objectivity worthy of science' (Hilts, 1978: 43).

But Abrams' account not only fails to understand *aliis exterendum* as a strategy and in the context of the times, it also fails to grasp the Society's own understanding of their work more generally. A closer look at the Society's annual reports from 1838 onwards suggest that it would not be correct to describe the Society's work as a-theoretical just because the Society had employed as its official strategy *aliis exterendum.*

The 1836-7 annual report of the council expressed its growing concerns that 'the exclusion of all speculative matter from the enquiries of the Society deters many persons from offering their personal assistance' (Bonar and Macrosty, 1934: 39). The Society was slowly beginning to realise that if the majority of the statisticians were also working within other fields, such as political economy, it would be hard, if not impossible, for them to totally separate their statistical work from their substantive work. An anonymous hostile critique published in 1838 in the *London and*

Westminster Review forced the Society to reconsider its position – the anonymous author argued that *aliis exterendum* prevents the Society from discovering new truths, deprives their labours from 'definite purposes' and turns their work into '*charlatanerie'* which would be 'a mockery and a lie' to 'all the ends of science and usefulness' (Unknown Author, 1838: 50, 68). The critique pointed to 'the impossibility of adhering to the rule – of adhering to it and advancing statistical knowledge at the same time', because statistical facts and tables would be useless unless they help in supporting or proving *something* (Unknown Author, 1838: 68).

Thus already in 1838, that is four years after it was established, the Society was making a clear distinction between opinions or speculation and theory deduced from empirical data:

> Like other sciences, Statistics seeks to deduce from well-established facts certain general principles which interest and affect mankind; it uses the same principles of comparison, calculation and deduction: but its peculiarity is that it proceeds wholly by the accumulation and comparison of facts, and does not admit of any kind of speculation […] It is not, however, true that the Statist rejects all deductions, or that Statistics consists merely of columns of figures; it is simply required that all conclusions shall be drawn from well-attested data, and shall admit of mathematical demonstration (Statistical Society of London, 1838: 3).

It remains unclear whether the distinction between opinions and theory was something that the Society implicitly had in mind in its earlier publications; what we do know is that from 1838 onwards, the Society was quite clear about this distinction and, as I show below, continued to make the case for it in many of its annual reports.

Also, there are noticeable similarities between the Society's understanding of the limits and procedures of their own work and the contemporary understanding of limits of science – namely, that scientific thinking proceeds first with the collection of empirical evidence and then engages with deduction from that evidence, not vice versa. But the deductions on the basis of 'mathematical demonstration' for which the Society allowed (nowadays better known as statistical modelling) were possible at the very basic level at this stage – we can now recognise the origins of statistical modelling in Quetelet's *Treatise* (1842, [1835]), but the mathematical procedures that enabled statisticians to model social phenomena on the basis of statistical data did not develop till much later. For Hilts, the fact that the Society allowed for

deductions on the basis of 'mathematical demonstration' did not make much difference because of the limited mathematical development of statistics at the time – with this allowance or without it, 'everything of a theoretical nature' was 'excluded' from the work of the society (Hilts, 1978: 37). However, this interpretation is rather extreme – the average of a characteristic of a population is still an example of statistical modelling, albeit a most basic one; comparing averages can still tell statisticians more about social change than if they did not have access to this information (and, we should remember, gaining access to this information was an extremely time consuming and laborious process). Adhering to strictly scientific procedures may have posed certain limits to what the statisticians could do in the 1830s, but it enabled them to achieve unquestionable progress *in the long run* when the mathematical procedures for inferential statistics were developed.

In the 1840s and 1850s, the Society continued to make explicit its position towards using theory as opposed to opinions. Examples from the annual reports and minutes of the Society show how lucid their understanding of theory was and how close it came to our modern understanding of the role of theory in scientific investigations:

> Such was not the duty assigned to this society by its founders; - it was not to perfect the mere art of "tabulating" that it was embodied; - it was not to make us hewers and drawers to those engaged on any edifice of physical science; - but it was that we should ourselves be the architects of a science of sciences […] the first prospectus of the Society announced its intention carefully to exclude all "opinions" from its publications; not assuredly with the view of discouraging the proper use of *a priori* reasoning or of hypothesis […] but for the purpose of devoting its publications to facts, not systems (Statistical Society of London, 1840: 1-6).

> […] the Council feel themselves called upon to congratulate its Fellows upon […] the augmented estimation in which they [statistics] are held as tests to and checks upon hypothetical reasoning in the moral and political sciences. […] Errors as to the facts which illustrate the actual condition and prospects of society are thus daily exploded and more just data are supplied for the exertions of the philanthropist, the judgments of the legislator and the speculations of the reasoner. The latter is compelled to amend his conjectures as often as they are irreconcilable with facts newly established by scientific observation (Statistical Society of London, 1843: 89).

> […] a Society like ours which admits not merely tables but a discussion of those tables […] We may proceed like mere children, accumulating pebbles on the sea shore and heaping them into one useless mass, or we

> may accumulate facts under the guidance of sound principles and make our accumulations more like the collections of the mineralogist or geologist putting together the various fragments he collects, but putting them together in order – collected with a view to an ultimate purpose. (Statistical Society of London, 1851: 101-4).
>
> It is always important to keep in mind that the perusal of such (statistical) tables in only one element towards acquaintance with the subject…. A person knowing nothing more than the table, would be, in fact, constantly drawing false conclusions […] (Lord Harrowby, 1851 quoted in Bonar and Macrosty 1934: 81).

These examples show that whereas opinions were supposed to be excluded from the work of the society, theory was not. They also show that, long before the *aliis exterendum* motto was dropped in 1856, the thinking within the Society had developed in a different direction. Theory was conceived not as a grand system of speculative principles but as something to be checked by statistical information gathered systematically. Put in the context of the nineteenth-century understanding of modern science, the statisticians were advancing the scientific understanding of society to a considerable degree, and, more importantly, much more successfully than any other contemporary social science project based solely on abstract theory, such as Comte's 'sociology' (see below). To accuse the Society of a shallow and naïve empiricism is to misconstrue their desire to put social science on a solid scientific basis in line with the standards of the time, to overlook their ambitious methodological rigour and to demean the practical contribution of their enquiries.

These examples also indicate the overall significance of the *aliis exterendum* debate – it prompted the statisticians to reconsider their own position, but also to better define the role of the statistician in society. It also helped reinforce the view that arguments that have no empirical basis cannot be accepted as valid for the purpose of science. It is easy for us to criticise the early work of the Society because we have come to embrace this view in many areas of study and in our everyday lives – but back in the 1830s this understanding was still an innovation, which, however, was gradually becoming common practice due, at least in part, to the Society's ambition and efforts.

One clear example of just how beneficial the *aliis exterendum* debate was for making the statisticians and their audience think about, and re-think, their understanding of the use of empirical evidence comes from the 1838 anonymous

critique that I quoted earlier. The critique takes as an example an article by G. R. Porter in which he attempts to argue that people who can read and write are less likely to commit crimes. The unknown author proceeds to challenge Mr Porter on the following grounds:

> The argument of Mr Porter is, that […] the uninstructed in reading and writing containing the most criminals – the best instructed, the fewest; therefore the reading and writing are the causes of the inferior number of crimes and criminals […] His object is to prove that one is the cause of the other; he assumes that he does so when he shows that they co-exist in the same classes […] But there are many causes of a moral nature which adequately account for fewer crimes being among those who can read and write well than among those who cannot. The classes best instructed in reading and writing are *also* best instructed in morals […] (Unknown Author, 1838: 67).

This critique contains one of the earliest examples of applying the idea that correlation is not causation, to the study of social problems. To this day, this idea is one of the pillars of statistical thinking and essential for the construction of statistical models. The invention of the techniques through which one can measure correlation and the impact of one social phenomenon upon another came much later with the work of Francis Galton and Karl Pearson. However, this example shows very clearly that the lack of mathematical techniques does not necessarily mean lack of deep understanding of how statistical, and social, analysis should proceed. And this is where the long-term significance of the debates surrounding *aliis exterendum* lies – by bringing to the fore the consideration of the role of theory in empirical statistical enquiries, the debate encouraged the statisticians and their audience to think more deeply about their work, and what would bring it closer still to the way science was conducted at the time.

As I pointed out earlier, an important part of the contemporary idea of science was the belief that it should have a popular side to its character – reaching out to ordinary people and establishing scientific networks of which the BAAS itself was a prime example. The statistical movement took a similar shape – although I have focused on the SSL as the major and longest surviving institution, in the 1830s-1850s numerous statistical institutions were established across Britain, including the statistical department of the Board of Trade (1832), the General Register Office

(1836/7), the Manchester Statistical Society (1833) as well as numerous other provincial statistical societies in Glasgow, Bristol, Belfast, Newcastle and elsewhere (for a full record and more details, see Cullen, 1975). Many of them were short-lived but they were representative of a new way of thinking about how science should be done.

Adolphe Quetelet's lifetime projects are an even clearer representation of a shift in the understanding of science, and social science, as a team-effort (rather than the work of lone individuals). Throughout his life, Quetelet worked incessantly to establish statistical organisations across Europe and hosted the first General (International) Statistical Congress. **'**As much as social physics was influenced by ideas of probability and progress', Quetelet's latest biographer argues, 'it was grounded in new forms of collaborative and professional data collection that marked a break with the Enlightenment understanding of scientific research' (Donnelly, 2015: 111).

Neither *aliis exterendum*, nor the work of the Society, was ever all about facts and nothing else, as Abrams argued; it was about improving the scientific understanding of society to garner better facts that could then be used to find better solutions to difficult problems. The novelty of the Society's work and the novelty of the debates inspired by that work is easily missed by focusing on isolated statements that the Society made (Abrams), instead of looking at what these statements *meant*, both in the short and long term. The long-term contribution of both Section F and the early work of the SSL is that they both worked in unique circumstances that produced unique debates, which were not possible before. Moreover, the lessons learnt from these debates have since become the foundation of our modern, empirically based study of society. Just one example of that is Quetelet himself:

> Like Quetelet, we believe that explicating the principles of social order is the precondition for effecting rational social change. Like Quetelet, we also assume that the foundation for comprehensive knowledge is the ordered collection of data. But again like Quetelet, we believe that the order in question is external to the observer, both in the usual sense of not being his creation and in the more restricted sense of its apprehension being beyond the reach of purely empirical investigation. Our crucial premise, and Quetelet's, is that the discovery of order, but not its fabrication, requires the use of analytic processes based in mathematics (Buck, 1981: 21-22).

It may be true, as Goldman (2002: 243) argued, that all social science projects that emerged in the early nineteenth century – Comtean positivism, socialism, Marxism, collectivism, laissez faire liberalism and statistics – thought they were scientific; but it is only of the statistical movement that we can say that it came close to being scientific in practice, as far as this was possible in the conditions in which they worked and according to the contemporary understanding of science; it was only statistics that *could* distinguish between facts and values, because the avalanche of printed numbers had made this possible; it was only statistics that proceeded first with active collection of empirical evidence and then followed, as best as they could, the principle of basing their enquiries and arguments upon empirical evidence; it was only statistics that accepted the scientific understanding of the role of theory in an empirical enquiry; and it was only statistics that embraced and helped to further the understanding of science as a public and shared activity across countries and across individual subjects.

Once the early work of Section F and SSL is put into the context of the contemporary understanding of science and also the contemporary social and political context, it becomes clear that what they were doing was neither unusual for their times nor inadequate as Abrams argues. Understanding the early developments in social statistics in Britain contextually and with reference to the contemporary understanding of science also helps to put up a convincing case of why the 'absent synthesis' of statistics and 'sociology' *matters.* Goldthorpe's analysis of the factors which contributed to an 'absent synthesis', to which I referred at the beginning of this section, has been very useful in enhancing our understanding of the interaction between statistics and 'sociology' in the nineteenth century; but it is only now that we are in the position to understand why this 'absent synthesis' should form a crucial and indispensable part of British sociology's history.

Chapter Five

The Role of Auguste Comte's 'Sociology' in the History of British Sociology

It is easier to recognise the significance of the contributions of Section F and the SSL in hindsight but at the time there were other ideas within social science movements which were influential and which did not recognise the work of the statistical movement as social science. Of the other movements that existed at the time (examples include Marxism, socialism, laissez faire liberalism) there was one philosophy whose ideas and orientation towards statistics was going to play an important role in the future development of academic sociology in Britain: the social philosophy of Auguste Comte, or Comtean positivism. Auguste Comte is well known as 'the father of sociology' – he was the first to introduce the term 'social physics' (1911 [1822]) and also 'sociology' (1839). His sociology was later to become the core philosophy of Britain's first Sociological Society. But there are other important reasons why Comte's philosophy and its reception in Britain are significant in the history of British sociology.

Comte's Positive Philosophy and his Idea of Sociology

Auguste Comte first introduced the term 'social physics' in an essay in 1822 but it was only in his *Course of Positive Philosophy* (1830-1842) that his ideas about a new social science were developed in great detail. At the centre of Comte's philosophy was the idea that all sciences progress from a theological stage (during which scientific knowledge is based on the understanding that the universe is governed by the direct will of supreme beings, or gods) to a metaphysical stage (in which it is no longer gods that direct phenomena, but forces and powers) until eventually they reach a positive stage. The positive stage is 'distinguished from the ancient […] by nothing so much as its rejection of all inquiring into causes, first and final' and in which research is confined to the invariable relations which constitute

natural laws (Comte, 1875: 511). In his *Course,* Comte proposed that social physics, or sociology as he eventually called it[5], will be the last positive science to emerge: at the time he was writing, social science was still in a 'theologico-metaphysical infancy' (Comte, 1875: 68) but, Comte believed, his work would help others 'to recognise the character of positivity in social as in all other science, and to ascertain the chief bases on which it is founded' (Comte, 1875: 2). The purpose of sociology was to extend the application of the 'positive principle' to the study of social phenomena and thereby help 'to discover the natural laws of a final order of phenomena, remarkable in the extreme and never before examined in this way' (Comte, 1875: 51). Sociology was to supersede previous and more rudimentary attempts to develop social science, such as in the work of Montesquieu or Condorcet, who did not make much contribution beyond recognising the possible existence of invariable laws in the social world.

Sociology would also help unify all existing attempts to study society, as, Comte argued, 'there can be no scientific study of society […] if it is separated into portions, and its divisions are studied apart' (Comte, 1875: 81). He repudiated the attempts of political economy to study political and economic phenomena in isolation from all other social phenomena; because of its narrow focus, political economy was of 'a merely metaphysical character, dressed up with special forms and a list of scientific terms' (Comte, 1875: 61).

But the utility of sociology went beyond the study of the social realm – in Comte's analysis, sciences are classified according to the degree of complexity of the phenomena they study. In this classification, the first science to reach a positive state was mathematics, then astronomy, physics, chemistry, biology and finally, sociology. As each science depends on the discoveries made in the science preceding it, sociology has the important task of:

> […] completing the whole body of philosophy, and showing that the various sciences are branches from a single trunk; and thereby giving a

[5] Comte introduced the term 'sociology' to replace 'social physics' when he found out that Adolphe Quetelet used social physics to describe his own work for which Comte had little appreciation: "I must above all signal the abuse with regard to the first term [social physics], which was adopted by a savant belge in recent years as a title of a work with is about nothing more than simple statistics" (Comte, 1839 quoted in Donnelly, 2015: 203).

> character of unity to the variety of special studies that are now scattered abroad in a fatal dispersion (Comte, 1875: 121-2).

Since sociology examines the most complex phenomena of all, it is not only the last science to emerge but also the only science that will have a complete scientific grasp of the laws governing *all* phenomena, while the knowledge of mathematicians would be of the most limited type:

> If, then, scientific men should stand forward to represent the positive attainments made in their respective sciences, the sociologists would be the only ones who could be regarded as having a complete knowledge of the positive method, while the geometers would have a more imperfect conception of it than any others, precisely because they know it only in its rudimentary state, while the sociologists alone would have carried it out completely (Comte, 1875: 500).

An important part of Comte's discussion of sociology was his views on the logic of enquiry appropriate to sociology. According to Comte, the deductive approach on its own was not sufficient to produce positive sociological knowledge; the inductive approach was also necessary. But, as John Stuart Mill pointed out in his review of Comte, while previous scientific thinkers, such as Jeremy Bentham, regarded it as proper to first deduce positive sociological laws from the laws of human nature and then use induction to verify these sociological laws, Comte argued for the reverse approach: 'in sociology, it is specific experience which suggests the laws, and deduction which verifies them' (Mill, 1866 [1865]: 85). Consequently, Comte argued, 'no real observation of any kind of phenomena is possible, except in as far as it is first directed, and finally interpreted, by some theory'; and also that 'all isolated empirical observation is idle, and even radically uncertain' and that 'science can use only those observations which are connected, at least hypothetically, with some law' (Comte, 1875: 97). For Comte, therefore, although observation was a necessary element of sociological enquiries, it played a secondary role in social science, 'subordinate' to that of the 'statical and dynamical laws of phenomena', i.e. the laws that determine social evolution (Comte, 1875: 99).

Comte does not specify what the precise objects of sociological observation should be; but from his discussion on the proper method for sociology it is understood that observation, and then the verification of that observation through

deductive laws, are going to be applied to the history of humankind. In Comte's view, prior to sociology, history had 'the character of *annals*' and as a result had not attempted 'the discovery of those laws which regulate the social development of the human race' (Comte, 1911 [1822]: 214). This would be the methodological task of sociology: to perfect the historical comparison between societies and between different historical ages in the development of humankind. Comte argued that because the method of historical comparison 'must prevail in all studies of which the living organism is the subject' and do so 'in proportion to the rank of the organism'; and because sociology's object is the most complex organism of all – the social organism – the method of historical comparison should be regarded as the 'chief scientific device' of sociology (Comte, 1875: 101, 105).

Differences between Comte's Idea of Sociology and the Statisticians' Idea of Social Science

This brief overview already suggests that there were major differences and incompatibilities between Comte's idea of sociology and the social science project of the social statisticians in Section F and in the SSL. Both Comte and the statisticians insisted upon a close relationship between natural science and social science. However, while Comte argued that sociology was the natural result of the laws governing the intellectual development of humankind which was manifested in the transition of science from a theological to a metaphysical to a positive stage, the statisticians argued that their social science was a product of their age, of the particular social conditions that emerged as a result of the industrial revolution that had prompted the gathering *en masse* of statistical data. The Comtean idea of sociology was *not* a response to a particular set of social problems; in contrast, statistics in nineteenth-century Britain developed as a direct response to social problems. This would have profound consequences for the self-identity of sociologists, as opposed to statisticians, throughout the twentieth century – while the former would regard themselves as holders of a unique intellectual power to reveal the general principles that govern, or condition human society, the latter would see

themselves as helping to illuminate the interaction between social phenomena responsible for social change.

There were further, more specific, differences between Comte's idea of sociology and the statisticians' idea of social science. As I explained above, Comte did not exclude the element of observation from sociology's methodology; however, for him, observation meant primarily the observation of social evolution through historical sources for the purpose of comparison and, as such, it played a subordinate role to that of the deduction of sociological laws from the laws of human nature, in which the ultimate verification of any observation should lie. Comte repudiated:

> the practice of reducing science to an accumulation of desultory facts, asserting that science, as distinguished from learning, is essentially composed, not of facts, but of laws […] (Comte, 1875: 511).

This view is in striking contrast to the view of the statisticians, who, together with Herschel and Whewell and other eminent scientists, argued that social enquiry proceeds first with the collection of empirical evidence about social phenomena and that laws about society are then deducted from these data, rather than from some abstract principles of human nature. For the statisticians, empirical evidence was prime; it was empirical evidence, not abstract deduction which eventually determined the veracity of any hypothesis. For the statisticians, the collection of statistical data was a welcome and extremely powerful and potent innovation; for Comte 'the deepest want of modern society is, in its nature, eminently theoretical' (Comte, 1875: 51) and statistical data collection was a grand and vain delusion:

> No examination of facts can explain our existent state to us, if we have not ascertained, by historical study, the value of the elements at work; and thus it is in vain that statesmen insist on the necessity of political observation, while they look no further than the present, or a very recent past (Comte, 1875: 108).

The differences between Comte's idea of sociology and statisticians' idea of social science boil down to a difference in their understanding of social change – while Comte understood social change as a grand historical phenomenon resulting from the evolutionary development of humankind, statisticians understood change

statistically, as a result of the co-variation of social factors and changes in the mode of individual interactions between people: while the former could only be studied with reference to history, the latter could only be studied on the basis of large amounts of statistical data and, later, through statistical inference.

But these two social science projects were not merely different or incompatible – for Comte, what statistics had to contribute to sociology was *irrelevant*; and for the statisticians, what sociology had to say about social evolution was also *irrelevant*. They were competing not only on two different grounds – they were competing on two parallel, but separate grounds. And, most importantly, each could ignore the other without consequences for its *own* credibility and development. The positions that Comte's sociology and the statistics of Section F and the SSL took with regard to one another in the nineteenth century set a powerful precedent for generations of sociologists and statisticians to come.

Comte's Views on Statistics

The fact that Comte's idea of sociology and the idea of social science developed by the statisticians in Section F and the SSL were seen as irrelevant to one another, does not mean that there was not a sense of hostility between them. What is more remarkable and significant, however, is that even this sense of hostility failed to engage the proponents of each idea in a dialogue. The following examination of Comte's views on statistics, as well as the reception of Comte's sociology in Britain provides some kind of explanation as to why this proved to be the case.

Comte was critical of any attempts to analyse social phenomena on the basis of principles which used mathematics. In the same 1822 essay in which he introduced the term 'social physics', he also discussed in detail his views on previous attempts to establish a social science, especially those which included a mathematical approach. One such approach, was the social mathematics branch of Marquis de Condorcet's plan for a social science. Although Comte admired Condorcet (he saw his own work as a continuation of Condorcet's 'moral-science-as-history' (to use

Hacking's (1990: 35) phrase)), he was adamantly opposed to Condorcet's idea that the calculus of probabilities can be of any use in sociology:

> […] the application of Mathematical Analysis is in no degree necessary to render Politics a positive science […] such a mode of regarding Social Science is purely chimerical and, consequently, altogether erroneous (Comte, 1911 [1822]: 193).

Moreover, in Comte's view, the useful application of mathematical analysis in sociology was 'impossible' because one of the 'leading characteristics of the phenomena peculiar to Organised bodies', such as society, was their 'extreme variability'; this extreme variability 'excludes all hope of ever submitting them to real calculations, such as for example, as those relating to astronomical phenomena' (Comte, 1911 [1822]: 194). Comte explained further that it would only be possible to apply mathematical analysis in sociology when 'everything really interesting would have been discovered', in which case the application of mathematics would be totally unnecessary and 'would no longer have any real importance' (Comte, 1911 [1822]: 198).

Comte's discussion preceded Quetelet's treatise (1835) by more than a decade. Quetelet had invented the concept of the 'average man' and had elaborately demonstrated how mathematical analysis can be successfully utilised not only in the empirical study of society but also in the discovery of general laws about society. So in that sense, it could be argued that Comte was simply unaware of what was possible when he was writing in 1822. His later writings, however, show no change of position towards the application of mathematics or statistics in sociology; if anything, his hostility increased. In the *Course* he mocked the attempts of 'some geometers to render social investigations positive by subjecting them to a fanciful mathematical theory of chances' as a mere 'pretension' (Comte, 1875: 120-1) and 'an extravagance which is wholly incompatible with the true positivity' (Comte, 1875: 501). In Comte's views, the theory of probability, in the Laplacian subjective sense in which Quetelet understood it, was an 'absurd doctrine' that 'has undergone no improvement, except in some matters of abstract calculation which it has given rise to; in fact it was 'impossible to conceive of a more irrational conception' (Comte, 1875: 120-121). And last but not least, all those social scientists and mathematicians

who advocated the use of mathematical and probabilistic analysis in social science were characterised by a 'disgraceful ignorance' for not admitting that the 'comparative method proper to biology, and the historical method proper to sociology, are the two greatest of logical creations' (Comte, 1875: 501) achieved in science.

Therefore, Comte's sociology as a positive social science did not involve the incorporation of statistics or the theory of probability. But what matters more than Comte's own views on mathematics, statistics and probability is their *legacy* – by proposing a system in which statistics was regarded as irrelevant and useless in the production of valid knowledge of society, Comte created a strong obstacle that could influence the thinking of British scholars *regardless* of the state of the development of statistics and probability. Just as Comte could ignore the advances made by Quetelet, later followers of Comte in Britain could ignore the later advances in mathematical statistics, no matter the opportunities they provided. In his argument, Comte did not cite any concrete examples, nor make direct reference to studies that utilised a mathematical approach of some kind, his objections remained largely abstract in nature but what matters is that in the long term he provided an argument and an excuse for social thinkers to ignore or dismiss mathematics or statistics w*hile at the same time claim authority over social scientific knowledge.*

The Reception of Comte's Views in Britain in the Nineteenth Century

Comte's sociology cannot be said to have been so widely adopted in Britain as to create a more or less united generation of thinkers; nonetheless, Comte's work 'was scarcely mentioned in French literature, when it was already working powerfully on the minds of many British students and thinkers (Mill, 1866 [1865]: 2). The reception of Comte's sociology in Britain was mixed: scholars like David Brewster (1838), J. S. Mill (1866 [1865]), J. K. Ingram (1878), Frederic Harrison became well-known supporters; while others, such as William Whewell (1866), W. S. Jevons (1875), Henry Sidgwick (1885), T. H. Huxley (1869a, 1869b) were strong opponents. Others, like Herbert Spencer produced 'sociological' work which could

be viewed as following in the footsteps of Comtean reasoning; but Spencer was very clear that he conceived of himself as being a 'dissenter' of Comte because of differences in their views regarding the classification and development of science (cf. Spencer, 1864).

One of the first influential accounts of Comte's work in Britain was written by J. S. Mill in 1865. In his *Auguste Comte and Positivism,* Mill was generally appreciative of Comte's work – he did not challenge Comte's laws of three stages in the development of science, nor did he question Comte's classification of the sciences. The issue about which he was most critical, was Comte's argument about the logic underlying the method appropriate to sociology. While Comte insisted that sociologists should proceed first with historical observation which would only afterwards be verified by appealing to abstract general laws deduced from what we know about human nature, Mill was of the opinion that social science 'is an abstract science and its appropriate method is the *a priori* method' (Mill, 1877: 143-4). According to Mill, social scientific knowledge could not possibly proceed 'while we look at facts in the concrete, clothed in all the complexity with which nature has surrounded them'; any attempts to 'elicit a general law by a process of induction from a comparison of details' would be 'altogether inefficacious […] as a means of arriving at any considerable body of valuable truth' (Mill, 1877: 148-9, 146). Despite these differences, like Comte, Mill expressed his strong reservations about the possibility of utilising the mathematical theory of probability in the study of social science: 'It would indeed require strong evidence', Mill argued, 'to persuade any rational person that by a system of operation upon numbers, our ignorance can be coined into science' (Mill, 1973-4 [1843]: 1142- Appendix F). He found the calculus of probabilities was based on profound misconceptions, which to some extent justified the extreme views of such 'a profound thinker, M. Comte' (Mill, 1973-4 [1843]: 1142- Appendix F). For Mill, improving one's observation was a much sounder strategy than relying on the theory of probability to compensate for one's ignorance or insufficient observation (Mill, 1973-4 [1843], Book III, Chapter 18, par. 3). Therefore, although Mill and Comte differed in their views about the proper logic of enquiry in social science, their strategies had one important thing in common: both Mill and Comte regarded the work in which the social statisticians were

engaged, which proceeded from the collection of empirical data to the formulation of statistical laws and which was closely associated with the calculus of probability, (cf. Quetelet 1849 [1845]) as inappropriate and unfit for the purpose of social science.

Just a year after Mill's review of Comte was published, another prominent thinker, William Whewell, expressed his views on Comte's philosophy. Whewell was one of the founding members of Section F and a strong proponent of an inductive political economy. Although he rather quickly became disillusioned with the work of Section F, he was nonetheless a supporter of an evidence-based social science in which statistics had a role to play and was strongly opposed to Comte's *idea* of social science and his positive philosophy. Whewell could 'in no degree share Mr. Mill's admiration for Auguste Comte' and saw Comte as 'a person whose want of knowledge and of temperate thought caused his opinions on the philosophy and history of science to be of no value (Whewell, 1866: 353). Most of the review which Whewell wrote focused on what he argued were the inadequacies of Comte's philosophy, calling Comte's idea of the law of three stages 'a radical mistake' (Whewell, 1866: 354). Whewell was also critical of Comte's vision of sociology, concluding that 'the reader may read any page of his speculations to see how superficial he is' (Whewell, 1866: 356).

Whewell's involvement with social science was brief and did not involve direct engagement with social scientific enquiries. But comments from other scholars, who were much more closely involved in social science reveal the same tendency that we observe in Mill and Whewell – association with statistics usually went with opposition towards Comte's sociology (and vice versa).

A further example comes from the economist and statistician William Stanley Jevons, President of Section F in 1870. Jevons was one of the best-known economists in Victorian Britain; he used his statistical and economic knowledge to produce a statistical atlas, as well as to establish the foundations of the mathematical approach to the study of economic questions in *A General Mathematical Theory of Political Economy* (1862). Although there are some important, even paradoxical, limitations to Jevons's work (cf. Stigler, 1999) he is nonetheless one of the first economists to engage in serious economic analysis based on the careful consideration of statistical data. In 1875, after years of involvement with statistics,

Jevons wrote a review of the second edition of the English translation of Comte's *Course on Positive Philosophy*. The review was critical, with Jevons raising three major objections to Comte's work.

First, he objected to Comte's claim to have offered any new insights into the development of science, describing Comte's law of three stages as 'one of those vague hasty generalisations which have the worst scientific vice of being incapable of precise verification' (Jevons, 1875: 492). One could stretch it 'like india-rubber' if one had to cover any difficulties with the analysis. Jevons also found that 'Comte had very wide and general views as to the possibility of creating great bodies of social science' but nonetheless he did not 'have any true conception of the proper way of going about the work' (Jevons, 1875: 492). A main reason why, according to Jevons, Comte had no proper idea as to how to go about doing social science was his rejection of the use of statistics and probability:

> he altogether abjured and ridiculed that branch of mathematical science, namely, the theory of probability, by which alone we can approach the scientific investigation of the complex condition of a nation. […] It becomes hardly possible to treat Comte's pretensions seriously when we contemplate this intellectual freak by which he rejects the theory which is becoming more and more the basis of exact science (Jevons, 1875: 492).

Attacks on the Comtean philosophy coming from eminent scholars such as Whewell and Jevons did not preclude others from associating themselves with Comte's positive philosophy and insisting on it as the one proper way of doing sociology and any other social science. One such follower was J. K. Ingram, an Irish economist and poet, who became the President of Section F in 1878. Ingram's address is regarded as one of the most memorable in the history of the section – partly because of its unusual proposals, partly because it was a response to a crisis that took place within Section F in the previous year. As I explained earlier, there was much discussion in the late 1870s in which Section F was accused of having failed to produce scientific work of high standards and the reaction to this failure came close to shutting it down in 1877 (Galton 1877a; Farr 1877;). Amid heated debate, it was decided that the Section should continue but that a reassessment of its approach and standards was urgently needed. A year later, Ingram's address provided one suggestion as to how this might be achieved. He argued that what he

perceived to be the greatest problem of economic science – its exclusive focus on the economic aspects of social life – could be remedied by re-organising the Section as a sociological section grounded in the Comtean philosophy. According to Ingram, political economy would 'cease to command unless it is subsumed under and absorbed into sociology' (Ingram, 1878: 614), bringing all social scientific studies into one unified science and replacing their methods with the historical method.

But despite the boldness of Ingram's sweeping suggestion, there is nothing in his paper that suggests how these ideas could be put into practice and no convincing argument that taking such actions would actually turn political economy into a 'scientific' subject. Towards the end of his address, Ingram expressed his disapproval of the importance that the Section had reserved for statistics:

> […] it is plain that though statistics may be combined with sociology in the title of the section the two cannot occupy a co-ordinate position. For it is impossible to vindicate for statistics the character of a science, they constitute only one of the aids or adminicula of science (Ingram, 1878: 628).

And with a particular reference to the statistical work of the Section, Ingram was clear that:

> more frequently, social statistics are used not to assist us towards scientific generalisations but as subservient to the direct practical action of the statesman (Ingram, 1878: 628).

However, it must also be acknowledged that Ingram was not totally dismissive of statistics: later on in his speech he stated his belief that 'the importance of statistical enquiries will rise as the deductive method declined in estimation' (Ingram, 1878: 628).

Ingram's address stirred up some interest among the wider public; but, significantly, no one in the Section appears to have considered Ingram's suggestion a feasible option; instead, the proposal was largely met with harsh criticism. Although subsequent attempts were made to broaden its focus, Section F was not reformed as Ingram had suggested. His speech, and the Comtean philosophy on which it was based, were severely criticised in 1885 by the political economist, Henry Sidgwick

who was then President of Section F. Sidgwick strongly disagreed with the arguments that 'sociology […] is really an established science' (Sidgwick, 1885: 611-12). According to Sidgwick an established science could be characterised by consensus, continuity and prevision and sociology did not meet any of these criteria. Sidgwick compared Comte's, Spencer's and Schäffle's (the leading social thinkers in France, Britain and Germany at the time) views on religion, industry and social evolution, exposing the great discrepancies between the works of these three scholars – they were a clear sign that sociology 'must be still in its infancy' (Sidgwick, 1885: 613). It was 'only too evident', Sedgwick continued, that:

> each philosopher has constructed on the basis of personal feeling and experience his ideal future in which our present social deficiencies are to be remedied; and that the process by which history is arranged in steps pointing towards his Utopia bears not the faintest resemblance to a scientific demonstration (Sidgwick, 1885: 613).

In addressing Comte's work on sociology, Sidgwick was adamant that it was an example of:

> how completely the delusive belief that he [Comte] had constructed the science of sociology, could transform a philosopher of remarkable power and insight into the likeness of a crazy charlatan (Sidgwick, 1885: 615-6).

Comte's sociology was later criticised again by Farrer, President of the Royal Statistical Society in 1894, who denounced the potential and scientific credentials of sociology. Other records show, somewhat ironically, that the ideas in favour of widening the focus of political economy as part of its reform agenda, also contained in Ingram's speech, helped strengthen the position of political economy, and *not sociology*. As Adelman (1971) argued, by the 1880s, 'the *economic* lessons of Comte had been absorbed by most professional economists':

> We have to thank the Comtist criticisms for forcing us to remember that the material truth of economic principles depends on complicated social conditions and that they have no independent validity (Cunningham, 1889 quoted in Adelman 1971: 186).

Comte's sociology was never accepted within Section F or within any other statistically minded circles. The main representative bodies of Comtean philosophy in Britain in the nineteenth century was the London Positivist Society, established in 1867 and the closely related English Positivist Committee, whose President (1880-1905), Frederic Harrison, was also president of the Sociological Society in 1910. These institutions endorsed Comte's social philosophy but failed in their attempts to popularise it and achieved virtually no success in developing and adapting it to the British social, political and intellectual scene (Bryson, 1936). Notably, none of the people involved in the positivist circles were proponents of statistics – this was especially the case with Harrison who, as we will see in the next part, was adamantly critical of statistics in his Presidential address to the Sociological Society in 1910.

The aim of this review of the reception of Comte's philosophy and sociology in the UK has been not to evaluate the strength of the arguments of Comte's supporters or his opponents in Britain but to show that those who supported Comte were generally sceptical or opposed to the idea of statistical social science; and those who attacked Comte's views were generally open to, if not always strong advocates of, the use of statistical and probabilistic knowledge in social science. With the introduction of Comte's views into Britain, a clear and remarkably consistent demarcation between these two sets of views was now possible. The few instances where Comte's followers addressed statisticians and vice versa show how difficult it was to reconcile both views. But they also show that there was little desire to do so – Comte's followers and the statisticians were two separate groups of people that threw criticisms at one another without engaging in meaningful dialogue. This consistent mutual un-attraction characterised the exchange of views between both groups throughout the nineteenth century; but it also set a remarkably persistent precedent that sociologists and statisticians followed all through the twentieth century.

Regarding each other's work as irrelevant; a lack of dialogue; throwing monologue-like critiques at one another or simple indifference are only some of the things that marked the interaction between Comteans or proponents of sociology and statisticians in both the nineteenth and, as we shall see, throughout the twentieth century. Of course, this is not to say that Comte's particular views, or the views of the statisticians in Section F or the SSL, *caused* the mode of interaction we observe

between sociologists and statisticians in Britain then and later. But it is important to recognise that although Comte's positive philosophy was largely forgotten by the mid-twentieth century, his *example of a sociology* still lives with us, even now: *Comte proposed the first systematic and coherent idea of sociology which rejected quantification*.

The impact that Comtean philosophy has had on British academic sociology (see Part Two) has also masked the contribution of the statistical tradition that had a more fundamental and long-term influence on social studies. The pioneer of the study of the history of science, George Sarton, once remarked that a 'great injustice is made when Comte is called the founder of sociology, for Quetelet has better claims to this title than he' (Sarton, 1935: 14). According to Sarton, while Comte was the first to speak of social physics and sociology:

> Quetelet was not only saying what to do, but was actually doing it, and doing it much better than Comte could imagine, for the real differences and the crucial points only appear when one is tackling concrete problems and tackling them in large numbers humbly and patiently; they remain almost always hidden from the superficial and superstitious minds of philosophers […] Comte was building proud castles of sands, Quetelet humbler constructions on bedrock (Sarton, 1935: 14).

We have seen that Quetelet did enjoy great recognition among scientists even during his lifetime; but he has never been hailed as an important figure in sociology, at least not in British sociology. To this day, few British sociologists would even know who Quetelet is, let alone regard him as a founding father of their subject. Auguste Comte, in contrast, may indeed have built merely 'proud castles of sands' that sociologists have stopped visiting since the 1950s; but even though he did not create sociology, and even though what he created did not endure for long, Comte showed that *it is possible to create a sociology of a non-quantitative type in which the main and superior element is theory of society.* And it is precisely because he opened up this possibility that Comte's sociology remains a crucial factor in the history of British sociology.

Chapter Six

Methodological Development of Statistics in the Nineteenth Century and its Application to Sociology

It can be argued that Comte and his followers in Britain regarded statistics as irrelevant to sociology because, even though statisticians were striving to follow the principles of contemporary science, statistics did not develop a great deal *methodologically* until the late nineteenth century. A closer look at what was possible to do with statistics and what was not, during the nineteenth century, will be useful here.

In the 1830s, Quetelet developed the idea of the average and showed how it can be used to describe masses of social data in a way that made possible a new kind of knowledge about aggregations of data and statistical regularities that could not possibly be known merely by looking at individuals. The idea of averaging had been around before Quetelet but he was the first to fully grasp its wider applications and propose its systematic use for the analysis of data *en masse*. The idea was revolutionary, at the very least because it is based on an apparent paradox that 'given a number of observations, you can actually gain information by throwing information away!' (Stigler, 2016: 4).

Not surprisingly, there was much opposition to averaging information about human beings, largely based on the assumption that this would mean the elimination of the free will of individuals[6] (cf. Donnelly, 2015; Hacking, 1990; Porter, 1986). In medicine, eminent doctors such as Claude Bernard argued adamantly that nothing could substitute a doctor's knowledge of his individual patients and that therefore averaging would be of no use (cf. Hacking, 1990); while in economics, some economists insisted that it was absurd to average data on the prices of different commodities in order to measure how prices change and therefore opposed W. S. Jevons proposition for index numbers of prices based on aggregate data (Stigler,

[6] In contemporary terminology, this kind of assumption that knowledge about an individual can be deduced from knowledge about the average characteristics of the group to which an individual belongs is called an ecological fallacy. It was not until the twentieth century that statisticians could properly explain why their work does not mean the end of all individuality and free will, exposing what were once seen as legitimate fears as fallacious reasoning.

1999, 2016). Regardless of the opposition, the application of the method of averaging flourished and became common practice in many areas of research, even physics: a few decades after it revolutionised the study of society, the method was used by James Clerk Maxwell to explain how molecules in gases move (cf. Porter, 1986; 1994).

But for all the potential uses the method of averaging offered, it was not enough to fully describe an aggregation of data – the average could tell scientists what is most common about a certain dataset, but it could not say anything about the way in which the data are dispersed. Aggregation is also limited because it does not say anything about the level of uncertainty of the conclusions based on the comparison of averages. For instance, in 1840 G. R. Porter had enough aggregate data to show that crime rates are smaller in areas occupied by better-educated people which led him to the conclusion that education could help eliminate crime. However, the comparison of averages that he used to back-up his argument could not tell him how certain he could be that this measure would work and how much of criminality could be accounted for by factors other than the level of education. We now use the theory of probability to measure this uncertainty, but as Stigler points out, 'the application of probability to the measurement of uncertainty in the social sciences was unknown before the 1870s' (Stigler, 1986: 194).

Thus throughout most of the nineteenth century the work in the SSL was confined to the limited opportunities that the comparison of averages could offer. Even some of the most prominent and mathematically gifted statisticians involved in the SSL conducted their work at the basic level of analysis, as originally suggested by Quetelet. A good example is W. S. Jevons, about whom Stigler says:

> If there was any nineteenth-century empirical social scientist who could have been expected to develop the techniques of the theory of errors into tools for the quantification of uncertainty in social sciences, it is W. Stanley Jevons. But he did not (Stigler, 1999: 88).

Another potential methodological innovator could have been William Guy. However:

> […] even Guy's work, which as late as 1881 was acclaimed as the 'work of the principle writer on the methods of statistics' was devoted to the nature and accuracy of the average and would now [1933] be regarded as elementary (Bonar and Macrosty, 1934: 178).

One has to be cautious of judging the limits of knowledge of the early statisticians in the light of the more advanced techniques that came later. However, a review of the papers published in SSL between 1834 and 1908 reveals a general lack of interest in the methodological development of statistics. Table 1 in Appendix I contains a summary of the topics of the papers published in the *Journal of the Statistical Society of London* in this period, showing that between 1834 and 1885, there was very little interest in methodological issues compared to other topics. This only changed after 1885, when, among others, F. Y. Edgeworth (1885, 1893) began developing mathematical statistics and Arthur Bowley (1906) developed the application of sampling in the British context; both of them publishing their contributions in the journal.

However, the greatest advances in the mathematical development of statistics towards the end of the nineteenth century came from Francis Galton who introduced the idea of regression (Galton, 1877b; 1886) and correlation (Galton, 1888). With regression, Galton was able to explain how amounts and frequency of deviation from the average on a given characteristic of the population can remain stable through time. With correlation, knowledge of the ultimate causes of larger scale events was no longer essential – it became possible for large-scale phenomena to be conceived of as knowable on the basis of their relationship with other large-scale phenomena. Both of these techniques were further developed mathematically by Galton's successor and the founder of biometrics, Karl Pearson, in the 1890s and early 1900s. Pearson also extended their application, by showing that they can be used to analyse and predict *any* kind of phenomena. But neither Galton nor Pearson were actively involved in the SSL: Galton had been a member of the SSL but made few contributions to the Society; and when in 1885 he discovered reversion to the mean (as he initially called regression), he presented his discovery not in front of the SSL or Section F, but in front of the Anthropological Section of the BAAS. Pearson was not even a member and had a low opinion of the kind of statistics that was done in the SSL, calling the statisticians there a bunch of 'dilettantes' (Pearson, 1894).

There has been some speculation as to why the most fundamental statistical innovations came from within the circle of the biometricians and why the SSL (which became the Royal Statistical Society in 1885) had contributed so little to the methodological development of statistics. Magnello (2011) and Stigler (1986) have suggested that what made the innovations possible was Galton's, Pearson's and F. R. Weldon's engagement with the Darwinian theory of evolution and their attempts to find statistical support for it; since social and economic theories, at that time, did not offer as solid a framework for statistical work and testing as that offered by Darwin's theory, statisticians in the SSL/RSS had little motivation for going beyond the comparison of averages.

However, a question that is of greater relevance to the history of sociology is whether and how statistical techniques were being incorporated by sociologists once they had been discovered. If we assume that the little that could be done methodologically with statistics throughout the nineteenth century played some role in making statistics look irrelevant to sociologists like Comte and his followers in Britain, did this attitude change with the advances in statistics?

One way to find out is to examine sociological works from the late nineteenth century which also deal with statistics. An obvious example here is Emile Durkheim's work *Suicide: a Study in Sociology* (2002 [1897]), which is still regarded as a classic in early sociology. Durkheim, like Comte, was French but he quickly became well known in Britain – he was one of the contributors to the first British Sociological Society which was established in 1903 and was highly regarded in its circles. Looking at his engagement with sociology and statistics cannot explain the engagement of British sociologists with statistics at the beginning of the twentieth century but it can give us an idea of what to look for later on.

Durkheim's work *Suicide: a Study in Sociology* introduced concepts such as social forces *sui generis* and *anomie* to explain sociologically what was usually conceived of as a private act, and for this reason it is regarded as one the first examples of a truly sociological analysis. In order to reach his conclusions, Durkheim made some use of statistical data but this has rarely been a topic of scholarly interest. A closer look at the role that statistical data played in Durkheim's analysis, and the way Durkheim himself conceived of that role gives us a valuable

insight into why statistics *could* continue to be regarded as irrelevant to sociology *despite* important advances in statistical methodology.

Durkheim's analysis of statistical data on suicide was limited to the comparison of averages, regardless of the fact that, at the time he was writing, Francis Galton had already published his discovery of correlation (Galton's works had not been translated into French but Durkheim read English and often referred to original works by other English authors, such as William James). However, correlation was still closely associated with a particular research context, that of evolution and heredity, and since it would have required some specialist mathematical skills to understand it, it may be assumed that Durkheim could not have seen its immediate relevance or even if he did, he may not have possessed the necessary skills to apply it. Even if we assume that this was the case, there is little doubt that the fact that Durkheim made use of *elementary* statistical analysis had the effect of making his sociological explanation appear more powerful than any explanation based on statistics. In addition, in his work, Durkheim made no suggestions whatsoever about possible ways in which statistical data collection or analysis can be improved for his purposes or more generally. This kind of attitude – using the contemporary limits of statistical analysis to put forward an argument that other non-statistical methods *have* to be used because they appear to be better – is an attitude that would continue to manifest itself in British sociology throughout the twentieth century (see Part Two and Three).

Durkheim was, however, in the position to discuss the *concept* of the average, as it had been developed by Quetelet, and was very critical of it. 'The average man does not kill himself' argued Durkheim; he emphasised that a completely novel explanation of suicide is necessary (Durkheim 2002 [1897]: 266). If this was the case, why did Durkheim use statistics at all?

Durkheim's use of statistics had a dual role. First, he used statistics to legitimise the conceptualisation of suicide as a 'collective', rather than private phenomenon; (Durkheim 2002 [1897]: 100); and to show that suicide is manifested through a statistical regularity just like other, more 'normal', human phenomena (Durkheim 2002 [1897]: 273). It is crucial, but remained *unacknowledged* by Durkheim, that, without statistics, he would not have been able to know anything

about suicide beyond anecdotes about *individual* suicide cases. There cannot be a sociology of suicide without knowledge about its large-scale occurrence, i.e. without statistical data on suicide rates. Durkheim's treatment of statistics represents another aspect of the kind of attitude that is commonly found in British sociology later on – of accepting the existence of statistical information without acknowledging how indispensable it actually is for simply knowing what is going on in a society.

But there was another important reason why Durkheim used statistics. Durkheim argued that comparing averages is useless in helping sociologists *explain* the source of suicidal behaviour (Durkheim 2002 [1897]: 266-7). Durkheim compared suicide rates in countries with, for instance, different religions and climates, concluding that relying on such statistical comparisons does not provide adequate explanations of why people commit suicides. He introduced a variety of social forces, such as *anomie*, to explain, sociologically, the occurrence of suicide. By employing statistical techniques in his argument, only to criticise them afterwards, Durkheim was, in fact, trying to legitimise the need of a purely sociological explanation of suicide; he repeatedly used them only to prove that, if they had any explanatory power at all, it was not sufficient. Statistics may help sociologists see, but it could not help them to perceive, suicide.

However, it is questionable why Durkheim did not consider it even possible to incorporate statistics in his sociological arguments as a form of building block; and why he considered it more reasonable to establish sociological arguments in opposition to, and on the basis of the complete rejection of, statistical knowledge and analysis. There was nothing inevitable about this decision – it would appear that Durkheim took it primarily because statistical laws were regarded as a type of natural law, while what Durkheim wanted to show was that suicide was a product of distinctly *sociological* factors. Durkheim's treatment of statistics demonstrates another important aspect of the kind of attitude that could be found in British sociology later on – the understanding that sociology had to substitute and supersede statistics; that what is known statistically can be better known sociologically.

This chapter began with a question about the importance of the history of nineteenth-century social science in the history of British sociology – if there were no 'sociological' societies or 'sociological' departments or 'sociologists' before 1903, when the first Sociological Society in Britain was established, then why should we study other nineteenth-century social science projects as part of the history of British 'sociology'? I explained how different scholars have approached the question; and I focused particularly on the only sociological account, that by Philip Abrams, which remains to this day the most widely read work on the history of British sociology among sociologists in Britain. In his essay, Abrams argued that the significance of the development of nineteenth century social science, such as social statistics and social reformism, lies in the fact that they frustrated the development of sociology, depriving it of institutional opportunities to develop.

It has been a primary goal of this chapter to expose the historical inadequacy of Abrams' interpretation of the role of the nineteenth-century statistical movement (Lawrence Goldman (2002) has done this with regard to Abrams' interpretation of the role of the social reformist movement). I have examined the development of statistical and probabilistic thinking and techniques and showed that in the early nineteenth century, the aims and structure of the statisticians' work, as well as their own understanding of their contribution to knowledge, was closely related to the contemporary goals and understanding of science. Put in the context of the contemporary understanding of science, the events and discussions that took place in the two most prominent statistical organisations, Section F of the BAAS and the SSL/RSS, are an example of the very first attempts to establish precise, reliable, widely applicable and generalisable scientific knowledge about the social world. This was crucial since the power of science, in principle and in practice, comes from the fact that science works, not from the prestige that science may have at a particular point in time (science will continue to work regardless of whether it has prestige or not). Thus, the fact that the statisticians followed in the footsteps of science is important not because they were trying to associate themselves with a prestigious tradition and prestigious organisations, but because they were engaging

in social investigations that sooner or later produce knowledge that works. To focus on the limited success of Section F to separate values from facts and on the difficulties of the statisticians in the SSL to keep up with *aliis exterendum*, as most scholars have done so far, is to miss a point of longer-lasting significance – that the development of statistics made such discussions possible in the first place; made social science possible. And this is why the question about the relationship between statistics and sociology is historically important; this allows us to question British sociology's attitude towards statistics in a way that is historically justifiable. An account of the history of British sociology that does not pay attention to these vital breakthroughs, and the context in which they occurred, is bound to be limited in what it can say about British sociology's general history. And Abrams' essay is a good example of this: the statistical movement did not frustrate the emergence of 'sociology'; it made social science possible, of which Abrams' implicit notion of 'sociology' – a primarily theoretical kind of study – is just one possibility.

In addition to failing to make explicit his assumptions about sociology's nature, Abrams also failed to recognise that the notion of sociology that he himself held originated in the nineteenth century in a social science project that received a great deal of attention in Britain at the time and since then has played a vital role in setting the conditions in which a sociology in Britain *could* develop. Comte's sociology followed different lines of reasoning and different methods from statistics – it emphasised the importance of a grand theoretical understanding of social evolution and its use of empirical evidence was confined to the use of historical records for the purpose of comparison. The legacy of Comte's work in the British context does **not** come from the fact that he somehow determined the later course of British sociology; the legacy of Comte's sociology lies in establishing a coherent precedent for a non-quantitative, non-statistical sociology despite the fact that, ever since the nineteenth century, statistical thinking has formed a crucial part of the basis of scientific thinking more generally. The early reception of Comte in Britain was indicative of the beginning of another influential trend – we saw that Comte's followers were at best sceptical and at worst hostile to statistics; but we also saw that the statisticians had very similar views about Comte's sociology and that both groups rarely engaged in dialogue with one another. Thus the emergence of the statistical

and the Comtean projects for social science are important as two social science frameworks which are not only mutually incompatible, but also capable of existing together in a state of mutual indifference and disregard for each other.

Apart from setting the institutional conditions and intellectual possibilities that existed prior to the establishment of sociology in Britain, this nineteenth-century overture to the history of sociology in Britain had another very important role. It served as an historical justification for examining the relationship between statistics and British sociology and why any conclusions about British sociology, more generally, which have been reached by looking through the prism of this relationship, deserve serious attention. This chapter showed that, by the end of the nineteenth century, statistics had not only paved the way for a social science closely following contemporary scientific practice, but had also provided it with a sophisticated methodology. With this methodology, statisticians could collect, summarise and, most importantly, assess the reliability of their data; they could get to know, but also control and predict, social and natural phenomena. However, the brief overview of Emile Durkheim's milestone sociological work *Suicide: a Study in Sociology* indicated clearly that the social science called 'sociology' was heading on a different journey. Here, statistics played the role of a ladder that could be discarded immediately after sociologists had climbed up to their destination; concentrating exclusively on where they've arrived rather than how they could get there in the first place.

This raises important questions about the twentieth-century development of sociology in Britain. Examining whether British sociology followed examples such as Comte's or Durkheim's with regard to statistics goes beyond any subjective curiosity about the relationship between statistics and sociology; the contribution of statistical discoveries, such as the method of aggregation or averages, regression and correlation techniques, and random sampling is indisputable on the larger scale of modern scientific development that began in the nineteenth century. Therefore, it is historically reasonable to at least assume that as statistics was already beginning, in the late nineteenth century, to revolutionise the way we do natural science, it was going to provide a viable way of doing sociology that sociologists in Britain could develop for their own purposes. As we shall see, the story of British sociology's

relationship with statistics tells us much more than might first appear – it tells about British sociology's perpetual problems to define itself; about the consistent and systematic tendencies in its more general institutional and intellectual development; and about its peculiar ways of coping with the unusual challenges it faced in an ever-changing academic climate. The first such challenge was the establishment of a Sociological Society which is the focus of the next part.

PART TWO

SOCIOLOGY AND STATISTICS IN BRITAIN, 1903-1930

Introduction

The Sociological Society that was established in London in 1903 was not the first social science organisation to be established in Britain but it was the first institution to call itself 'sociological'. Nonetheless, no broader social movements, public events or even other institutions are known to have been connected to, or involved in supporting, the idea of a Sociological Society at this time: the Society that was formed in 1903 had only weak institutional links with all the major social scientific institutions that had developed previously in the nineteenth century.

The tradition of social reformist empirical enquiry developed by the Social Science Association (SSA) (1857-1886) was treated with indifference by the newly formed Sociological Society and *direct* links between the SSA and the Society are almost non-existent. In the archives of the Sociological Society in Keele the SSA receives two passing comments in the private documents of the Sociological Society's organiser, Victor Branford (Branford, 1903a; Sociological Society [Victor Branford], 1903a); aside from this, the Society, as an institution, made no acknowledgement of, and paid no tribute to, the SSA, neither to criticise nor praise its contributions.

Institutionally, the Sociological Society was also distanced from the Royal Statistical Society (RSS). The Sociological Society had its first meetings in the rooms of the RSS; and there were a few individuals who were members of both societies – but this is as close as the two institutions would ever be.

The London Positivist Society, established in 1867, and the closely related English Positivist Committee endorsed Comte's social philosophy which included a particular understanding of sociology as a general science that united all existing social science specialisms and used the comparative method to analyse the development of social evolution (Comte, 1875). The positivists, however, failed in their attempts to popularise Comte's views and achieved virtually no success in developing and adapting them to the British social, political and intellectual scene (Bryson, 1936). And, just as with the SSA and the SSL, direct institutional links between the Positivists and the Sociological Society are difficult to find, except for the fact that Frederic Harrison, who had been a president of the Positivist Committee

in 1880-1905 also became the president of the Sociological Society in 1910. If, then, the Sociological Society was, at least institutionally, largely unrelated to any nineteenth-century organisations, how did it begin?

The Sociological Society was established in 1903 on the initiative of Victor Branford, a business accountant who was a close friend of Patrick Geddes, the Scottish social evolutionist and city planner. It seems that Branford's and Geddes's own personal agenda played the primary role in its establishment – they wanted to use the Society as a vehicle for promoting Geddes' ideas and for securing him a university post in London (cf. Renwick, 2012). But this tells us only part of the story, since the motives the Society presented to the public officially were not only much broader in scope and ambition, but had a more enduring impact.

Branford's official invitations highlighted the fact that Britain lagged behind in the 'organisation of those general studies which – under the name of Sociology – are concerned with integrating the specialist studies of Man' (Sociological Society, 1903b). One of Branford's core aims, which he fulfilled, and which survives to this day in the form of *The Sociological Review*, was the establishment of a sociological journal as, unlike other countries, Britain lagged behind in this respect. There were also no sociology courses offered at university level[7] and, Branford argued, the time was 'ripe' to bring together those who seek to lay the foundations of clear thinking about social problems (Sociological Society, 1903b). The way the Society was planning to achieve this was by following the principles of sociology as outlined by Auguste Comte in his social philosophy: this is evident from the Society's official publications, which emphasise the establishment of sociology as a general science in the Comtean style (for further details, see discussion below). Further evidence comes from Victor Branford's own writings on the subject which are infused with references to the Comtean vision of sociology (Branford, nd [1903]; Branford, 1904a; Branford, 1905a, Branford, 1928). The Society also organised a meeting in 1907 to commemorate Comte's fiftieth anniversary. At these meetings, papers were read by Victor Branford, L. T. Hobhouse, Benjamin Kidd, J. A. Hobson; all of whom held major positions in the Society. The papers made it clear that, although Comte's

[7] A new piece by Christopher Husbands (forthcoming) reveals that there were a few sociological courses in the late nineteenth century; however, they receive no mention by Branford.

views were taken both critically and with approval, the Society overall was devoted to realising Comte' ideal of sociology (Sociological Society, 1907). In addition, prominent followers of Comte's social philosophy, such as J. H. Bridges, S. H. Swinny and Frederic Harrison, held high positions at the Sociological Society, with Swinny being a Chairman of the Council (1908-11) and Frederic Harrison taking up the position as President of the Society in 1910.

While the society embraced, as its intellectual credo, Comte's positivist view of sociology, they exhibited a dismissive attitude towards the intellectual contributions of the SSA and the SSL. Intellectual links with the social reformist or the statistical movements, which were engaged with what could be described as primarily empirically oriented work, were not part of the organisers' plans: according to Branford, the Society had a higher goal, a vision of sociology that went beyond anything that could be achieved through empirical work: 'We want to be able to say to the empirical sociologists – "there is a certain position in sociology, reach it and you will be amply rewarded" (Branford, nd. [1903]).

The first official meeting of the Society took place on May 15th/16th 1903[8], in the rooms of the RSS, with social anthropologists, social theorists, social workers, politicians, economists, and others, present. An important figure was James Martin White, a Scottish businessman, who financed the founding of the Society, the first sociological journal and the first sociology professorship. The official goals of the Society were 'scientific, educational and practical'. Following the Comtean vision of sociology as a general, all-inclusive science, the Society aimed 'at affording a common ground on which workers from all fields and schools concerned with social phenomena may meet'[9] (Sociological Society, 1905a). As a result, the Society had a very diverse membership. The three groups that made the most notable contribution in the Society's first few, and most active, years were – the eugenicists under Francis Galton; the supporters of Geddes's civics and a diverse group of social philosophers and reformers (cf. Halliday, 1968). Very soon, however, these three groups of 'sociologists' each went their own way, exposing the shaky foundations of the

[8] According to Branford, this is also the date, which is 'the most correct date for the founding of the society' although subsequent accounts in the Sociological Papers point to different dates in that year (Branford, 1926).
[9] The list did not include statisticians.

Society's organisation. After the eugenicists left to establish their own Eugenics Education Society; and the social philosophers established themselves at the London School of Economics and Political Science (LSE) under L. T. Hobhouse; Geddes, Branford and their allies continued to be the sole active force within the Society (cf. Evans, 1986). For the rest of its existence – until the 1950s – the Society operated on a small scale, virtually existing in name alone.

Studies on the history of the Society are lacking in historical detail and accuracy of interpretation and have left fundamental questions un-probed. For instance, the underlying assumption guiding Abrams' account is that what 'was good for social reform was bad for sociology' (Abrams, 1968: 106). As a result, his analysis of the Sociological Society is dominated by the idea that the Society would have been successful had it developed a general theory of society; it was due to the fact that neither Hobhouse, nor Geddes, nor Galton succeeded in this, that, according to Abrams, the Society ended up merely stimulating the 'fissiparous growth of pseudosociological factions' (Abrams, 1968: 119). In its emphasis on theory as the only element of social research that makes such research 'sociological', Abrams' account is not only presentist – applying a 1960s view of sociology to the past and judging it accordingly – but it is also dismissive of legitimate, and, indeed, as yet unexamined, questions regarding both the historical background of the Society and its long term significance: for instance, how did the Society's *choice* to take a different direction from the empirical tradition of social enquiry that developed in Britain during the nineteenth century affect the future of sociology in this country? And what was it that happened within the Society that contributed to its limited legacy?

Compared with Abrams, a 2007 debate published in *The Sociological Review* took a more positive and appreciative look at the Society's legacy by revisiting the contributions of Branford and Geddes. The debate aimed to 'recognise anew the importance of these forbears' (Savage, 2007: 429); reclaim the importance of the social theory of Geddes (cf. Studholme, 2007) and retrieve Branford from obscurity, praising him for 'envisioning' a 'theoretically and empirically grounded sociology' (Scott, 2007: 479). However, a comprehensive historical understanding of the legacy of these figures, and their role in the Society, should involve not only an examination

of their theories, ideas and vision, which *we* happen to see as relevant, but also an analysis of what they actually *did* with the opportunities that the establishment of the Society presented. Those theories endorsed by the leading figures of the Society certainly deserve our full attention but, somewhat surprisingly, the theoretical framework that has been left out of both older and more recent accounts, is Comte's theory of sociology which, as I show below, played a fundamental role in the decisions made by the Society.

The preoccupation with the Sociological Society's limited contribution to the development of sociological theory, to be found in older and more recent scholarship, has reinforced a particular view of the Society's position regarding the nineteenth-century statistical tradition of social enquiry. As already discussed in previous chapters, Abrams argued that as far as a relationship between 'sociology' and statistics existed at the time the Sociological Society was being established, it was a relationship in which the development of statistics and the consolidation of the statistical movement during the nineteenth century served to *frustrate* the development of 'sociology' in the UK. Abrams argued that the SSL had a negative influence on the development of sociology in Britain since its work was not theoretical but, instead, endorsed empirical inquiries. For this reason, Abrams did not question the fact that the Sociological Society distanced itself from the work of the RSS; after all, a Sociological Society had to be about sociology, not statistics. And in 'blaming' the 'failure' of British sociology on of British sociologists' inability to produce viable sociological theories, the arguments of other scholars such as Anderson (1968) and Soffer (1982) have served to spread the impression that statistics *was never a choice* for sociology and for the Sociological Society.

However, detailed archival research on the development of sociology in the first half of the twentieth century suggests that these interpretations are misguided and in need of improvement. Therefore, the following chapters examine in detail how it was that the Sociological Society took such a remote stance towards statistics: whether it was a result of a series of *choices* or whether it was driven by an inevitable course of events. This is **not** to argue that the Society *ought* to have embraced statistics; however, as explained in previous chapters, the fact that the nineteenth-century statistical movement represented the first attempt to study the

social world using the same logical approach which has resulted in major advances in the scientific study of nature since the 1600s and, ultimately, has shaped modern society, provides historically legitimate reasons to examine the factors that influenced the Society's stance towards statistics and how this stance was justified.

Chapter Seven

The Limited Contributions of the Sociological Society to the General Development of Sociology in Britain

I. First Formal Discussion about Sociology

The Sociological Society was founded with a view to realising in practice Comte's idea of sociology as a general science. However, at the time, Comte's philosophy was still seen by many as dubious and impractical; and there was much hostility and confusion surrounding the Comtean idea of sociology. Sociologists, it was claimed in 1904, were a disjointed 'company of mystics' in which 'every man has a different interpretation to give'; the general view was that there was not 'any united thought or concentrated view to be learnt from sociologists' and that 'one comprehensive science' would not take the social sciences very far (Speaker I in Branford 1904a: 124; Speaker C in Branford, 1904a: 122-123).

When the Sociological Society attempted to organise the first formal and official discussion about sociology in Britain – its first limited contribution – that discussion failed to resolve the problematic issues at the heart of the Comtean idea of sociology discussed above. The first President of the Society, James Bryce, simply assumed that their vision of British sociology was not only feasible, but well-grounded: 'I trust, however, that it will not be difficult to justify the Sociological Society' (Bryce, 1905: xiii). His justification was limited to repeating what was already well known – there was, as yet, no distinctly British sociological theories and there were no sociology courses taught in British universities. But Bryce made no attempt to explain why this was the case; why this situation should change and whether following Comte's idea of sociology could deliver that change.

Branford took a similar approach. He himself pointed to the 'hostility', 'indifference' and 'misunderstanding' prompted by the very word 'sociology'; and to the fact that while some acknowledged the need for a sociology but denied its relevance and legitimacy, others *totally* denied the possibility of a general study of

social phenomena (Branford, 1905a: 10). This is an apt *description* of the problems facing sociology but neither Branford, nor anybody else in the Society took on the task of effectively addressing these problems and proving the critique wrong in practice. Branford instead assumed that following Comte's principles was sufficient for the establishment of a theoretical and applied sociology as long as sociologists develop the ability to think both philosophically and scientifically (Branford, 1905a: 16). But how the cultivation of these abilities would come about, how these abilities would become manifest in the methodology of sociology and how sociologists would on the basis of these abilities address the problems of modern society – is not discussed. In addition, the articles in the Sociological Papers, presented to the Society between 1904 and 1907, did not engage at all with the idea of sociology as a general science – contributors, including prominent members such as Galton and Geddes, do not even mention how the specialist subjects of their own study would contribute to the establishment of the 'great' science.

The situation was not much different in 1907 when the most active period of the Society was coming to an end. In his inauguration speech, Edward Westermarck, the Finnish philosopher and ethnographer, who became a part-time professor of sociology at the LSE in 1907, acknowledged that sociology was still perceived as 'too vague' and 'too full of far-reaching but unproved generalisations'. He went on to confess that he considered 'these objections to contain a great deal of truth' but, like others before him, failed to suggest any constructive solutions (Westermarck, 1908: 26).

The only person who attempted to address some of these challenges and indicated a possible solution was the eugenicist and statistician Karl Pearson. Pearson was not an actual member of the society and had 'a certain want of sympathy' with it due to what he believed was the unscientific character of the subject (Branford, 1904b). Pearson reluctantly agreed to attend one of its meetings in order to preside over the reading of Galton's first paper. In his address, Pearson called the Society 'a herd without its leader', expressing grave doubts that the Society could function effectively and sustain itself without an established leading figure to 'set bounds' to sociology and 'prescribe its functions' (Pearson, 1905: 52).

Although Pearson's argument had eugenicist connotations – putting emphasis on *an exceptional individual* who will create the rudiments of sociology – his suggestions were plausible and perhaps closer to a feasible solution of the problems sociology was facing than any of Branford's ideas based on Comte's strategy of creating a unified sociology. It was not so much an exceptional individual *per se*, but the kind of coherent strategy, the pulling together and effective co-operation that such an individual embodies that was needed. Branford, however, did not accept Pearson's argument, calling it 'good theology', 'bad science' and 'a creational hypothesis' (Branford, 1905b: 26, 28) and re-stating that 'the progress of sociology is in quite normal fashion' and that 'all is well with our science' (Branford, 1905b: 40).

Yet what happened to the Society in the next couple of years, proved Branford's soothing remarks ill-judged and if it weren't an exceptional individual, then British sociology was certainly lacking something. The discussions that took place in the Society either could not agree what this thing was or failed to acknowledge it in the first place. It is clear that despite the fact that the Society revived the discussion about sociology, its members merely re-stated the problems that sociology faced, without contributing much to their solution.

II. Definitions of Sociology

The second limited contribution of the Society is closely related to, but distinct from, the first. The Society offered a friendly platform for discussing the nature of sociology and presented an opportunity to come up with a suitable definition of its subject. However, various factors interfered with the success of these attempts.

A selection of the definitions of sociology that appeared in a discussion in the Sociological Papers displays a remarkable diversity. It was claimed that sociology was 'a specialism under physiological psychology'; 'the science of history'; 'the same as philosophy of history'; 'the continuation of politics'; 'the Philosophy of the Social Sciences'; 'the science of General civilisation'; 'a *Method* applicable to all social sciences'; 'convertible with philosophy' and 'the science of social facts'; at the end it was even claimed that any attempt at a definition would be useless at the

current stage because sociology was not yet developed and 'definitions do not anticipate sciences, but they succeed them' (Various authors, 1905: 211-258).

The first report of the Society argued that the discussion, in which all these definitions appeared, successfully re-affirmed that sociology was 'the corpus' of all social sciences; an 'endeavour to synthesise the researches of all social investigation'; and an 'endeavour to construct a theory' (Sociological Society, 1905b: 20-21). But through what was a mere re-statement of the Comtean principles, the report obscured what had really happened in the discussion. A brief look at the definitions shows an obvious lack of agreement. A longer, more careful look, indicates even more remarkably, a lack of **dis**agreement: definitions coming from all directions, without a single unifying thread or sense of coherence, showing just how little the participants had in common with each other. The members of the Society did believe that there was something out there called 'sociology', like the eighteenth-century belief in a mystical Southern continent that was worth finding; but when asked where it lay, everyone pointed in a different direction.

None of these definitions suggest that sociology should involve itself in the direct investigation of social problems on the basis of empirical – statistical or any other – evidence. Instead, as quoted above, the emphasis is on the construction of theory. Sociologists, such as Abrams in the 1960s, who approved of this idea because it corresponded to *their own* idea of sociology, did not see this episode in the early history of sociology as problematic. But when we look at the Society's concepts of social science in the context of the previous history of social science in the UK, we see a fundamental rift between what was being proposed here and the tradition of social empirical inquiry in the UK that developed in the nineteenth century. The important point to grasp is not whether the Society's understanding was right or wrong; better or worse; but the fact that it was not the inevitable route, or even the already well-established route, in the British context. The Society may not have reached a consensus on what sociology was; but if their definitions exerted any long-lasting influence, it was to legitimise the *choice* that sociology in Britain was not going to be about empirical, let alone quantitatively based, investigation.

III. Sociology in Academia

The third limited contribution of the Sociological Society was the introduction of sociology into academia. Sociology was first introduced as an academic subject with this name at the London School of Economics (LSE) in 1904. Lectures were given by Hobhouse; Westermarck; A. C. Haddon, an anthropologist; and Lafcadio Hearn who lectured on Japanese civilisation. All but Hearn were members of the Sociological Society. In 1907, Hobhouse became the first full-time professor in Sociology, while Westermack took a similar part-time appointment. Hobhouse occupied the Chair till his death in 1929 and Westermarck – till 1930. For twenty-five years these two men *were* academic sociology in Britain[10] and with them they carried the conviction that lay at the heart of the Society, that sociology is a philosophical and a theoretical subject.

The First Sociology Courses

As the person who sponsored the first sociology lectures, James Martin White, a friend of Branford and Geddes, had an important say in what these lectures should entail. Martin White had decided to invest in sociology because he thought it was vital for sociological knowledge to be spread among the public and members of parliament, who, in his own experience, appeared to be ignorant of the subject (Sociological Society, 1904: 21). He recommended 'the study of the more general and philosophical aspects of sociological science' and felt there was a need to 'create a body of academic opinion in favour of re-organising the curricula of social studies' in such a way that they more adequately recognise 'synthetic sociological conceptions' (Sociological Society, 1903c). Unlike others in the Society, Martin White did not hesitate to emphasise that in addition to constructing 'a scientific theory of society', sociology should aim to indicate the bearing of such knowledge on practical life (cf. Martin White in Fincham, 1975: 32).

[10] Empirical social science was simultaneously developing in other circles, such as LSE's Social Science and Administration Department which was established in 1912 (cf. LSE Calendars, 1895-1979).

The ‘Martin White lectures’ were introduced according to plan in 1904. They focused on the study of ethnology and comparative psychology, social institutions and social ethics, essentially taking a Comtean approach – emphasising the comparative method and theoretical examination of the history and evolution of civilisation in different societies (LSE Calendars, 1904-1929). In addition to the Martin White lectures, in the period 1904-1930, under sociology, the university offered other, similarly oriented, courses as well as a course on *Logic and Scientific Method* a course on *Methods of Social Investigation*[11].

An insight into the understanding of sociology at the LSE and the role of the sociology courses in the social science curriculum can be garnered from the list of potential audiences. Despite their focus on social philosophy and abstract theory, in the LSE calendars the courses were not advertised as likely to be of interest to academicians but to professionals with a clear practical orientation, mostly future local state administrators (LSE Calendar, 1904). Sociology, therefore, appears to have played a kind of enlightening ‘liberal arts’, complementary role, to a variety of other core subjects, such as economics, finance etc. that would be preparing one for one’s profession.

Fincham (1975) points out that the final make-up of the sociology courses depended less on what Martin White, on behalf of the Society, had to offer as sociology; and more on the academic situation at the LSE. According to Fincham, the LSE was initially reluctant to accept sociology because they were already teaching many ‘sociological’ subjects, such as economics, social administration and statistics. The LSE, Fincham argues, had little incentive to re-organise existing courses leaving sociology no choice but to fill a gap in the curriculum, that of taking up the task of teaching social theory and social philosophy.

But Fincham overestimates what sociology had to offer at this early stage. Even if the Society wanted to compete with already established subjects at the LSE, the Society simply was not in a position to teach a variety of sociological specialisms

[11] The course on *Logic and Scientific Method* was largely philosophical. *Methods of Social Investigation* was more practically oriented but comprised of only three lectures on methods of sociological study common to other sciences (hypothesis, collection of data, statistics, verification) and peculiar to sociology (interview, documents, literature review). While *Logic and Scientific Method* was given continuously from 1905 to 1950, *Methods of Social Investigation* was relatively short-lived and was given only between 1905 and 1912.

– it did not have the people to teach these specialisms (there were no statisticians or social administrators heavily involved in the Society); plus there was a limited number of people prepared to take up the post and deliver on the Society's understanding of sociology – and those who were prepared, like Hobhouse and Westermarck, had already limited their work to the theoretical approach. As we saw earlier from the list of definitions of sociology, the Society had already agreed that sociology was not going to be about empirical investigation; there was little chance that they would have changed their mind and made a different choice, had the academic situation at the LSE been different. Paradoxically, in its attempt to encompass all social sciences and unite them in the Comtean mode, sociology had turned itself into a specialism that fulfilled the role of 'liberal education' – to provide broad knowledge of the world – a tendency that continued throughout the twentieth century.

Some members of the Society welcomed the fact that the Society was able to successfully promote a non-empirical approach at the LSE. J. M. Robertson, for instance, argued that the fact that empirical and reformist social science was already being taught under a different title at the LSE meant that sociology could devote itself to philosophical and comparative analysis and that this provided a good opportunity for British sociology to go beyond the empirical work on 'Drink Drainage and Divorce', as he sarcastically put it, that originated in nineteenth-century organisations such as the SSA and that, to his regret, had taken over American sociology (Sociological Society, 1904: 22-23). Similar views were expressed fifty years later at the closing meeting of the Institute of Sociology (established in 1930 as a result of the merging of the Le Play House and the Sociological Society; cf. Evans, 1986: 34), at which the empirical direction in which American sociology had developed was referred to as a 'mistake', which had led to American sociological departments being 'limited to the handling of concrete social questions' (Farquharson, 1957: 2). Ironically, American academic empirical social science had developed largely through the incorporation of empirical methods first developed in Britain; for British sociologists to brand it 'a mistake' at a time when American sociology was flourishing and British sociology was closing down one of its two representative institutions, the Institute of Sociology, demonstrates the

deluded views of at least some leading sociologists at the time. There is every reason to believe that these attitudes persisted within British academic sociology circles long after the demise of the Sociological Society and its filial institutions.

Hobhouse: Professor of Sociology

After Branford had given up the idea that Geddes would be a suitable candidate for the chair (cf. Renwick, 2012), his attention turned to Hobhouse. He privately told a close friend that 'something ought to be done to secure him [Hobhouse] for Sociology' and that his 'is the one personality round which the whole movement, at present inclining to be dispersive, might be crystallised and concentrated' (Branford, 1907).

Branford's motives in wanting Hobhouse were, it seems, largely driven by practical concerns. Geddes' failure to win support in the Society combined with the fact that, in 1907, Hobhouse had lost his job and had by then some experience of teaching sociology, opened up the possibility of Hobhouse's candidacy. However, when recommending him for the professorship, Branford did not point to Hobhouse's sociological contribution or academic competence; he emphasised that Hobhouse would more easily accommodate the variety of sociological views that existed in the Society, and contribute to their successful co-ordination due to the fact that he was less strongly committed to one particular approach to sociology. It is perhaps ironic that, after rejecting Pearson's criticism that the prospects for the Society are slim without an exceptionally capable individual, Branford's decision was based on Hobhouse's 'personality'. The professorship was, therefore, not so much a sign of the increasing strength of sociology in academic circles, as yet another attempt to *remedy* problems of conflict within the Society and, ultimately, deeper problems within British sociology itself.

A further indication of the weak and tentative beginnings of Hobhouse's career and, indeed of sociology as an academic subject, comes from Hobhouse's inaugural address. Instead of outlining his plans and ambitions for the future of sociology, Hobhouse talked about the 'roots of modern sociology'. The address was similar in

focus and purpose to the 'propagandist'[12] pieces of the founders of the Society. It repeated the well-known Comtean principle – 'the problem before us as sociologists is to bring together in vital connection the inquiries which hitherto have been pursued apart' (Hobhouse, 1908: 21) – but again, no suggestions are made as to the realisation of this ideal. When we also take into account Westermarck's inaugural address as a part-time Professor of sociology, delivered at the same time as Hobhouse's, in which he admitted that many of the criticisms sociology was facing were in fact accurate, it becomes difficult to view the academic establishment of sociology in Britain as a promising or auspicious event.

With the professorships established, Hobhouse and Westermarck began teaching sociology as a special subject that was initially part of the BSc Economics. Not till much later, from 1920, did it become a separate degree. Their lectures changed little over the period of their professorships – Hobhouse taught 'Social Ethics', 'Sociology and Ethics' and 'Social Evolution', while Westermarck taught on early customs and social institutions. Their courses had a wide scope in the sense that they discussed social phenomena in general terms; but they were not an example of sociology reaching out to other social science fields and methods, with the view of incorporating them into one comprehensive science. The open approach to teaching sociology which derived from the Comtean understanding of sociology as a general science resulted in a situation in which *any* social science topic could be taught under sociology. And, so it happened, that over the course of the next twenty years, sociology taught courses on subjects as diverse as India, ancient Greece, social psychology and social structure. The upshot of these first attempts to establish sociology in this country were that sociology remained an ill-defined and marginalised subject that failed to establish its own clear boundaries.

Apparently, Hobhouse himself did not feel at all confident in his abilities to promote sociology in academia. Up to 'the early years of the war' Hobhouse 'had been wont to speak despondently about his own lectures on sociology'; he complained of 'his failure to get the field of studies and research clear' and even indicated 'some thought of resigning the chair' (Branford, 1929: 276). The event that

[12] 'Propagandist' is Branford's own description of his writings in one of his personal letters (Branford, 1908a).

is said to have changed 'the sociological atmosphere' in Hobhouse's mind, was the completion of an encyclopaedic article on sociology (Branford, 1929: 276). Whether or not this article had a huge positive impact on Hobhouse's psychological state, the article had little impact on the state of British sociology within and without academia. Ultimately, it said nothing new about sociology – Hobhouse maintained that sociology was 'the synthesis of the social studies'; that 'it may be taken to cover the whole body of sociological specialisms' and that its 'object of discovery' was 'the connecting links between other specialisms' – all of which showed that sociology had changed or developed very little since it was first introduced into academia (Hobhouse in Hastings, 1920: 654).

Hobhouse's legacy is mixed – those, like his successor Morris Ginsberg, who value his contribution to social philosophy, maintain that he had the 'rare power of combining metaphysical speculation with detailed painstaking empirical investigation' and praise his work as 'the most comprehensive and successful attempt in recent times whether in England or abroad towards a systematic sociology' (Ginsberg, 1929: 144); while others, focusing on his role in establishing academic sociology, argue that his tradition of 'theory and encyclopaedism' was 'unfashionable' (Sprott, 1957: 609) and 'fatal to sociology in England' (Soffer, 1982: 792). Without committing to either of these rather extreme views, it is reasonable to say that Hobhouse's contribution was limited to *sustaining* the rather dormant existence of sociology at the LSE. Since the introduction of sociology into academia was mainly due to Branford, with the financial support of Martin White, *and not to the progress made by the Society,* the professorships contributed little to the expansion and popularisation of sociology and even less to the fulfilment of the wider ambition of turning it into a general science. As with its other contributions, the Society's attempt at establishing sociology as an academic subject had long-lasting consequences for British sociology largely because of its *limited* success, exposing once again the shaky foundations of the Society's idea of sociology and the difficulties surrounding its implementation in academia.

IV. A Sociology Journal

The fourth attempted contribution of the Sociological Society was the creation of a sociology journal. From 1904 to 1907, the Society published three volumes of the *Sociological Papers*; and from 1907 onwards, it began publishing *The Sociological Review* on a quarterly basis.

The founding of a journal on similar lines as the *American Journal of Sociology* or the *Revue Internationale de Sociologie* was seen as a 'chief function' of the Society (Branford, 1903b); as the achievement that justified its coming into existence, since 'English students of sociology were seriously handicapped for want of such journal' (Sociological Society, 1903d). In spite of the Society's conviction that the *Papers* would remedy a serious problem that needed urgent solution, the publication of the *Papers* was met with mixed reviews.

The press and a few popular foreign periodicals made positive comments, acknowledging that the *Papers* 'secured a footing in the scientific world' by 'its collection of expert comment from all the leading countries' and that they made a valuable contribution towards clarifying for the public the meaning of the term 'sociology' (Sociological Society, 1906). But the reception of the Papers in academic journals was more negative.

One British review acknowledged that the *Papers* brought together the work of social scientists from a variety of fields; but saw 'the miscellaneous and tentative character' of the contents as an indicator that the sociological literature was still uncoordinated (Jones, 1905: 440). Another review questioned the relationship between sociology and anthropology – 'in what relation do we stand to the professed sociologists?' and 'How is their field of work going to be distinguished from ours?' – and criticised the Papers for neither directly raising, not directly answering these questions (Burne, 1905: 120). An American review questioned the attempt to provide a common forum for sociologists: 'In England there comes together a body of specialists in one or another social study, who discuss many problems, each man from his own standpoint' (Davis, 1908: 152). And finally, a review in the *Journal of the Royal Statistical Society*, questioned the Sociological Society's reluctance to engage in statistical work:

> But surely a sociologist, biologist, or meteorologist who handles the statistical data of his science in complete ignorance of statistical method is quite as bad as a statistician who deals with the data of a science of which he has no special knowledge? (Yule, 1907: 518).

The idea of methodological co-operation between statisticians and sociologists was not completely absent from the discussions during the first few years of the Sociological Society (cf. Webb, 1907). But, it would appear that neither statisticians, nor sociologists, were able to give any clear indication of how this co-operation could be achieved in practice.

The lack of co-ordination and of any clear sense of direction which emerged from the *Papers* and was reflected in these reviews, continued to manifest itself in *The Sociological Review* in the years that followed. Under the editorship of Hobhouse, between 1907-1910, the *Review* published on diverse subjects like crime and magic, Islam, Indian agriculture, vital statistics and so on; thereby struggling to build up its own distinct style and character. Hobhouse's resignation from the editorship in 1910 was 'accepted with great regret' (Sociological Society, 1910) but with a general consensus that, despite his valuable work, his 'general line of editorial policy tended to depart from the scope and aims of the Society' (Branford, 1929: 276). Once Branford took over as editor, the last strong link between academic sociology at the LSE and sociology at the Sociological Society was broken and the Society was by now reduced to a small circle around Branford and Geddes. But the journal itself could not resolve the essential problems that sociology was facing, including how sociology would differentiate itself from the many specialist disciplines that were already in existence; and how it would organise specialist knowledge into a coherent whole. The journal may have stirred up popular imagination and enthusiasm about the sociological ideal of an all-embracing social science, but it fell far short of turning this ideal into a reality; in fact, the journal only served to show the difficulty, if not the impossibility, of such a task.

Chapter Eight

The Influence of the Sociological Society on the Relationship between Sociology and Statistics

The above discussion reveals how that the Society's long-lasting legacy stems from the limited advances they made in trying to organise a clear and coherent study of sociology within their circles, in their promotion of academic sociology and in the founding of a journal. However, there is another important aspect of the Society's legacy that has to do with the choices that were made with regard to creating potential links with the statistical tradition of empirical enquiry. This last part pays special attention to what happened within the circles of the Society to influence their particular stance towards statistics.

How the Sociological Society's Idea of Sociology Shaped Their Attitude towards Statistics

A good place to start is the Society's general view and understanding of sociology and its tasks. In its first three and most active years, between 1904 and 1907, the Sociological Society adopted the Comtean idea of sociology and, following Comte, the Society set themselves the task of providing 'common ground on which workers from all fields and schools concerned with social phenomena may meet' with a view to ensuring that co-operation between different groups of social scientists would improve understanding of social phenomena (Sociological Society, 1905a). Given this claim of taking a wide and open approach, it seems surprising that no statisticians were involved in the Society's work and that links, if they existed at all, between the Royal Statistical Society and the Sociological Society were extremely weak. So how was the Society's ideal of universal co-operation between social scientists to be achieved without reaching out to all possible groups of social scientists, particularly statisticians and economists?

As I have already explained, according to Comte, the application of mathematics and of the mathematical theory of probability to the study of society was 'inadmissible' and could be reduced to a mere 'pretension' on the part of 'some geometers to render social investigations positive by subjecting them to a fanciful mathematical theory of chances' (Comte, 1875: 120). For Comte the method 'proper to sociology' was the historical method (Comte, 1875: 501); the study of society through mathematics was not only incompatible with the principles of positive philosophy on which sociology should be based but it was also sheer 'extravagance' (Comte, 1875: 501). There is no evidence that any of the organisers of the Sociological Society strictly adhered to Comte's understanding of the use of mathematics and probability in sociology, but the fact that Comte clearly denounced the value of statistics in sociological enquiry would have made it difficult for anyone who followed his philosophy to look at statistics sympathetically.

Comte's understanding of *sociology*, however, *did* have a direct influence on the Society's attitude towards statistics and probability and their role in sociology. Comte's sociology was to be a general science that could offer something above and beyond the existing anthropological, psychological, economical and, indeed, statistical explanations of social phenomena. The Sociological Society set themselves the goal of turning this idea into a reality – to achieve unification of the social sciences both intellectually and institutionally. The establishment of a statistical sociology, building on the work already done in this direction during the nineteenth century, was, therefore, against the very ideal which the Society aimed to follow. The Society argued that:

> The important thing was to bring home to people the differentia in the handling of a given problem by Sociology as against its treatment by those specialist sciences which also dealt with sociological phenomena, especially such groups as Economics, Statistics and Demography; Anthropology, Folklore and Primitive History institutions; Psychology, Child-studies, Ethics; Social Pathology, Criminology (Sociological Society, 1903b).

Even though the Society struggled to clarify the distinctive features of the proposed new sociological approach, it is clear that, in these circumstances, developing 'a statistical sociology' would have been regarded as developing yet

another sociological specialism, rather than preparing the ground for the founding of a general science.

Although Comtean attitudes towards mathematical statistics and the Comtean idea of sociology may have created unfavourable conditions for the development of an explicitly statistical sociology, they did not totally exclude statistics from sociology – after all, the idea of sociology as a general science was supposed to include statistics, at least in some form. However, any prospects of the inclusion of statistics into sociology was stifled by the Society's lack of a clear and workable strategy regarding the way in which the idea of a sociology as general science could be implemented – about ways in which statistics, or any other 'social science specialism', could be turned into *a part of* sociology.

One way in which the inclusion of statistics into sociology as a general science could have been achieved in practice would have been to establish secure connections with the Royal Statistical Society. As it turns out, however, the only explicit link between the Sociological and Statistical Societies, was Edward Brabrook, who was chairman of the Executive committee of the Sociological Society in 1903 and Chairman of the Council in 1904 and had been Vice-President of the Statistical Society in 1900 and President of Section F (Statistics and Economics) at the British Association for the Advancement of Science in 1903. In addition, Sir John Macdonnel, Vice President of the Statistical Society, was present at the Council of the Sociological Society in November 1904. There is no evidence, however, that either Brabrook, or Macdonnel, or anyone else, made any significant efforts to improve the connections between the Statistical and the Sociological Societies.

This question was discussed in some detail by Beatrice Webb, who recommended that the Sociological Society proceed with care in order to avoid doing anything that might intimidate the existing social science institutions. Instead, the Society should 'strive to enrol in it the very best brains of the Royal Economic Society, the Royal Statistical Society, and all the others' (Webb, 1904: 25). However, the Society devised no strategy for action based on her recommendations.

Another possible way of incorporating statistics as part of a Comtean sociology would have been a close co-operation between the *Journal of the Royal Statistical Society* and the journal the Sociological Society was planning to establish. The latter

was supposed to fill an important gap since Britain still had 'no journal which continuously and systematically prosecutes the study of social phenomena in their totality and records the progress of such study in other countries' (Sociological Society, 1903a). This statement was, strictly speaking, true, since all social science journals that existed in Britain at the time were devoted to a specific branch of social science. But without any acknowledgement that the existing journals had been contributing to the corpus of sociological knowledge, such a statement also implied that the material contained in these journals, including the *Journal of the Royal Statistical Society*, was perceived as 'un-sociological'. Such an attitude of dismissal was unlikely to prepare the ground for future co-operation between the journals and their producers.

Endorsement of the Comtean idea of sociology, resulted in less favourable conditions for the development of a close, productive relationship between sociology and statistics in the UK than would have been the case had the Society endorsed, say, ideas based on the English empirical tradition of social enquiry which had been developing throughout the nineteenth century. But what created even bigger obstacles was the Society's lack of ambition and, indeed, willingness to establish secure connections with the statistical institutions that already existed in the UK. The fact that the Society made little effort to establish institutional co-operation with any other social science institutions, let alone the Royal Statistical Society, inevitably begs the question as to how genuine the society's commitment to establishing a general science of society actually was.

The Role of the Sociological Society's Idea of Methodology in Shaping Their Attitude Towards Statistics

The most straightforward way of incorporating statistics into sociology would have been as one of its methods. By examining the general discussion about the methods of sociology that took place in the first three and most active years of the Society, we can see whether statistics was part of these discussions, and if so, how its role was perceived.

The Society's early *Sociological Papers* (1905-1907) do not contain a specific discussion on methods. Even the details – advantages, flaws and application – of the comparative method, which was supposed to be the primary method in a Comtean-style sociology, was not discussed at the first meetings of the society. The only insight we can garner about the Society's attitudes towards any potential sociological methods comes from a variety of individual contributions.

In the paper, which Victor Branford, chief organiser of the Society, sent around to advertise the proposed sociological society, he argued that sociologists should possess the ability to think both philosophically *and* scientifically. 'The Sociologist', Branford argued, 'derives his general attitude, mental tendency and outlook from philosophy', but he or she should not neglect 'the ability to study society in the manner of the positive sciences' (Branford, 1905a: 16). This would require knowledge in the mathematical or physical or biological sciences, which would help them to apply precise observation and rigorous logic to their studies. However, there is no sign that the Society ever followed up these recommendations; instead we see the emphasis placed on philosophical work and the teaching of social philosophy (cf. the work of L. T. Hobhouse and Morris Ginsberg); statistical training did not become part of the sociology curriculum until the early post-war period.

Statistics is mentioned on a few occasions in the early *Sociological Papers* but the majority of comments show that the interest in it as a sociological method was superficial. For instance, in 1904 Branford acknowledged that there *was* such a thing as statistical sociology, founded, according to him, by Condorcet, De Witt and Quetelet (cf. Branford, 1904a: 111-112). But he did not explain how, if at all, this 'statistical sociology' would form part of sociology in the UK, and his comments had no practical results for the incorporation of statistics into sociology.

In their contribution to the *Sociological Papers* of the Society in 1905, Durkheim and Fauconnet wrote in a similar vein, but displayed a more sophisticated understanding of the role of statistics in sociology. According to them, the major contribution of statistics was to reveal how societies could be characterised by birth-rates, marriage-rates, criminal statistics etc., showing that these do not depend on 'the capricious will of individuals, but express social conditions permanent and definite whose intensity can be measured' (Durkheim and Fauconnet, 1905: 275).

Their paper further acknowledged that sociology is 'no longer the monopoly of sociologists alone' (Durkheim and Fauconnet, 1905: 276); nonetheless, sociology was and could only be 'the system of the social sciences' (Durkheim and Fauconnet, 1905: 268). In such a system, statistics would play some part; however, even Durkheim failed to make clear what this part would be and whether statistics would be considered a *sociological* method.

Other commentators were not even prepared to consider a place for statistics in sociology. John Ingram, the Comtean who in 1878 had made an unsuccessful attempt to reorganise the Economics and Statistics section of the BAAS into a sociological one, argued that statistics 'is not a branch of science at all, it is congeries of observations ancillary to several sciences' (Ingram, 1905: 235). This comment may have been coloured by Ingram's resentment at the way in which the political economists and statisticians had reacted to his suggestions for reforming the Statistics and Economics section of BAAS into a sociological one, a few decades earlier, but Ingram's views were influential and should not be ignored – he was the second best-known and most active follower of Comte in the UK after Frederic Harrison (Harrison was also very critical of statistics, cf. Harrison, 1910).

In addition, the *Sociological Papers* do contain a few positive remarks about the potential contribution of statistics to sociology. These comments at least show that some of the Society's members had a clearer vision about the possibility of incorporating statistics into sociology. For instance, Winiarski, a Swiss scholar and professor of political economy, argued that the social sciences are evolving towards a positive stage, characterised by better precision and accuracy, and better ability to formulate general laws. Similarly to Comte, he also argued that none of the 'special social sciences, in spite of their sturdy development, are able to formulate scientific general rational laws' (Winiarski, 1905: 252) and that this would be the subject matter of general sociology. However, in contrast to Comte, Winiarski also argued that the positive stage of the development of science, and general sociology itself, would be related to the further development and expansion of statistics:

> […] general sociology is still in the intermediate qualitative phase; but, subject to the evolutionary process which all sciences pass through, it must

> enter into the abstract quantitative phase, just as the concrete sciences will transform themselves into statistics (Winiarski, 1905: 253).

Unlike anyone else in the Society, Winiarski recognised that quantitative methods had the potential to improve general sociology – a view that in Britain at the time was shared only within the circles of economists regarding economics. His discussion, however, was limited to a rather abstract outline of the possible development of social science without giving any clear suggestions as to how this would be practically achieved. Moreover, there is no evidence that his view was widely discussed within the Society, suggesting that there was little enthusiasm.

Beatrice Webb's comments on statistics, however, did receive some general feedback and, therefore, give a clearer indication of the Society's attitude towards the role of statistics. Webb suggested that statistics is necessary in nearly all social observations in order to prevent 'falling into the fallacy of the individual instance' (Webb, 1907: 349). She argued that the potential of statistics to provide an insight into the spread, rate of occurrence or magnitude of the social phenomena should be utilised as far as possible, but she also argued that this should happen alongside the application of other methods:

> […] it is the qualitative methods of observation – experiment and the use of documents and literature – which enables one to discover the processes of society, and it is the statistical method that enables one to check these observations, to see that they apply to a large number of instances instead of to one only (Webb, 1907: 350).

Webb's paper is the only contribution to the *Sociological Papers* that contains a clear plan outlining the role of statistics in sociology. The paper, however, received a rather cold reception, which focused on the potential problems related to the practical implementation of her views, rather than on the advantages it could bring. The discussants were rather sceptical towards any interference on the part of the statisticians. It was argued that statisticians could not do sociological work, because their competence in collecting and classifying sociological facts 'was not beyond question' (Unknown, 1907: 352). The possibility of training sociologists in statistics and mathematics was not discussed. Instead, following these comments, Webb agreed that:

> […] the statistician should never be allowed to collect his own data; he did not understand that the work of the sociologist was delicate observation of processes, each of which was slightly different from the rest (Webb, 1907: 353).

However, Webb also added that this situation could be remedied by an effective co-operation between a statistician and a 'qualitative observer' (Webb, 1907: 353). The Society, however, did little to encourage such co-operation in practice. Overall, the unproductive and fragmented way in which the members of the Society discussed the methods of sociology and their ambivalence towards the utility of statistics meant that the establishment of the Sociological Society did little to improve the prospects for the development of a British statistical sociology.

The Role of Branford's, Geddes' and Galton's Personal Views in Shaping the Sociological Society's Attitude Towards Statistics

In addition to the intellectual and institutional factors discussed above, there were other factors related to the personal interests of the leading members of the Society, notably Victor Branford, Patrick Geddes and Francis Galton, that also played an important role with regard to the early prospects of a co-operation between statistics and sociology.

Victor Branford was the main organiser of the Sociological Society and the chief decision-maker. His attitude towards statistics would have been vital in fostering good relations between the Society and the statistical institutions that existed in Britain at the beginning of the twentieth century. At that time, the statistics community was represented by two separate and distinct circles of researchers: one circle developing the so-called 'new English statistics' centred around Karl Pearson and his colleagues; the other centred around the Royal Statistical Society. There are some indications in Branford's official writing and personal correspondence which suggest that he was sceptical towards the possibility, and indeed the value, of an effective co-operation with either of these circles.

For instance, Branford wrote a rather mocking reply to a criticism of the state of sociology that Karl Pearson had made at one of the meetings of the Society:

> We may anticipate that before long some industrious anthropologist will discover among "prehistoric remains" a curious document called "*Cours de Philosophie Positive"*. Seen as a survival of early culture, the document will arouse sufficient interest to be deciphered. It may even be comprehended. The statisticians will doubtless measure it, and – since there is no limit to the wonders of science – they may even read it (Branford, 1905b: 37).

Since, at the time he made these remarks Branford was in the throes of a heated argument with Pearson, it could be construed that his comments are targeted particularly at Pearson and the type of statistics he was developing and not at statistics in general. Perhaps, then, these comments create an exaggerated and somewhat distorted image of Branford's general attitude towards statistics in Britain. Nonetheless, Branford's decision to make churlish remarks instead of challenging Pearson's criticisms in a more constructive and professional manner, does suggest that his overall view of statistics was, to say the least, cynical.

Karl Pearson may have been the leading statistician in Britain at the time but he himself had abstained from joining the Royal Statistical Society, partly due to intellectual differences and partly due to Pearson's tendency to avoid affiliation with societies or clubs of any sort (cf. Pearson, 1894 for Pearson's criticism towards the Royal Statistical Society). Therefore, although Branford and Pearson fell out when the Sociological Society was established, there was still a possibility that the Sociological Society and RSS might work together. As it turns out, however, Branford's personal views of the RSS were also rather negative and his attempts to co-operate with them came to nothing. In a 1903 letter to Geddes, Branford mentioned that the secretary of the Statistical Society had unofficially 'declared his belief in the need of a special sociological society'. But establishing a good relationship with the statisticians proved difficult and frustrating:

> Since I last wrote to you about the attitude of the statistical society, I have been trying to force their hand by constantly pressing on them the need for purchasing more sociological literature, and as they haven't sufficient funds to buy all the purely statistical literature they want, they are put to

> the alternative either of straining their resources, or disclaiming the pretension to oversight of sociological interests. From a recent talk I had with the Secretary, I gather that they are inclining to adopt the latter alternative (Branford, 1903c).

It would appear, then, that from the very beginning the possibility of a productive relationship between the statisticians and those interested in promoting sociology was hindered by a lack of substantial interest in sociology on the part of the statisticians, on the one hand; and somewhat feeble efforts on the part of the sociologists to gain some practical support from the statisticians, on the other. Both Societies failed to get any closer than sharing the same building for a few months. Why this was the case is discussed in more detail in the next section.

An important insight into Branford's preferences for a particular type of sociology comes from a paper he wrote in 1904 on 'the founders of sociology'. In this paper, Branford discusses at great length some of the contributions of the Marquis de Condorcet. Condorcet has been mentioned briefly in the previous chapters; he is regarded by historians as one of the first to formulate the idea of a social science, even though he contributed little to its realisation. What is peculiar about him is that he proposed a social science of a two-fold type – to use Hacking's (1990) phrases, Condorcet conceived of social science as consisting of moral-science-as-history which examined grand scale social evolution; and moral science as social mathematics. Both Comte and Quetelet admired Condorcet but for different reasons: while Comte took up moral-science-as-history and transformed it into an idea of sociology, Quetelet took up Condorcet's social mathematics and transformed it into social statistics. As for Branford, he gave much praise to the idea of moral-science-as-history and argued that it is because of this that Condorcet deserves a place among the founders of sociology; he hardly mentioned Condorcet's idea of social mathematics. The Sociological Society's sociology was, of course, not Condorcet's moral-science-as-history; nor was early twentieth-century British statistics Condorcet's social mathematics. But one could not help thinking that Branford's choice to praise the former but not the latter as a foundation stone of sociology puts him, and with him the Sociological Society, in line with Condorcet the moral philosopher and Comte the positivist, rather than with Condorcet the social mathematician and Quetelet the social statistician. It is curious that what did not

seem a problem to Condorcet in the eighteenth century – the existence of these two types of social science in harmony and co-operation – was to prove such a difficult task to the founders of British sociology and to future generations of British sociologists.

Branford was not the only individual whose personal views would prove important for any potential co-operation between statistics and sociology. As Renwick (2012) argues, the Sociological Society was supposed to prepare the ground for the realisation of Patrick Geddes' sociological ambitions: one of the major aims of the Society at its outset had been to secure Geddes' popularity among sociologically minded intellectuals and thereby secure for him the first chair in Sociology. But Geddes too, it seems, had little interest in working to develop a statistical sociology that would include a close co-operation with the statisticians.

Geddes had an old interest in statistics. Back in 1881, he had given a lecture on the proper classification of statistics, in which he gave suggestions as to how statistics from different countries could be better organised into comparable units (cf. Geddes, 1881). In this paper, he also acknowledged that statistics has the potential to enhance and improve social scientific enquiries. By the time the Sociological Society was established, however, other substantive and methodological interests had overtaken his work. In his first paper given to the Society, Geddes argued:

> […] after many years of experiment and practice in teaching sociology, I still find no better method available than that of regional survey, historical as well geographical […] in my view, indeed, a sociological society has at least as much use for a collection of maps, plans and photographs as of statistics (Geddes, 1905: 108, 110).

This and the subsequent papers Geddes gave at the Society's meetings suggested that Geddes wanted to develop an *alternative* sociology that was not focused on, or limited to, well-established methods, such as statistics. The meandering and obscure style of his papers, however, failed to convince the members of the Society that the programme he was proposing was viable. But even if he had succeeded in persuading the Society to follow his plans, Geddes, it seems, would have done little to promote further co-operation between sociology and statistics: none of Geddes' other articles in the Sociological Papers even mention

statistics; statistics is also absent in the Sociology Lecture Syllabuses that Geddes compiled in 1905-1907, which take as an example the city of London and attempt to analyse it via geographical, anthropological, economical, aesthetical, political, historical, psychological, idealist and social pathological – but no statistical – approaches (cf. Geddes, 1905-7; 1906, 1907). Nor does Geddes' later work in the Sociological Society and at the Le Play house contain any reference to statistics.

Francis Galton was both a pioneering statistician with a distinguished reputation and, at least for a short time, a member of the Sociological Society. His involvement in the Sociological Society might have improved the prospects of an effective co-operation between statistics and sociology but this failed to happen for a number of reasons. Firstly, the correspondence between Branford and Galton (cf. Branford, 1902-1905) suggests that the relationship between the two men was rather superficial and that there was no intellectual bonding. Branford's letters to Galton, are notably pretentious, exaggerated and flattering:

> I need not say how welcome was your allusion to your expanding the paper somewhat […] is it [eugenics] not the very crown and summit to which historical and theoretical sociology must ultimately lead? (Branford: 1904c).

Except for the lavish praise of Galton and his eugenics programme that Branford expressed in his personal correspondence, Branford did not compliment or seriously engage with eugenics in any other official writing. It seems likely, therefore, that Branford was not genuinely interested in eugenics but was merely trying to secure for the Sociological Society the endowment that Galton was planning to make in order to promote research into eugenics (see Renwick, 2012 for more details). Nor, it seems, was Galton's interest in the Sociological Society and the sociology they were promoting, any deeper than Branford's interest in Galton and his eugenics programme. In their correspondence, Galton's brief and matter-of-fact hand-written notes stand in contrast to Branford's flattering letters. Galton writes mainly about the revisions and presentation of his papers to the Society. Although grateful for the opportunity Branford was offering him, he did not express much concern, let alone praise, for sociology. This egocentric attitude on Galton's part is also evident in the papers he read at the meetings of the Society and which form a major part of the

Sociological Papers. In his papers, which focus primarily on eugenics, Galton did not make a single reference to sociology or to the question of how eugenics research and its methods would contribute to general sociology – a clear enough sign that Galton had little or no interest in sociology for its own sake, but only in so far as it helped him in developing eugenics. More importantly, he does not discuss how statistics as a method in its own right could prove useful in sociology or how it could be incorporated into sociology, suggesting that Galton's continuous presence at the Society would have done little to improve co-operation between sociology and statistics. As it turned out, the audience at the Sociological Society was not impressed by Galton's plans for eugenics. Branford was forced to send his personal apologies to Galton for the cool reception and unsupportive attitude, putting the blame on his own failure to 'bring forward a larger number of statisticians and biologists' whom he had invited but who failed to attend (Branford, 1904d). Regardless of Branford's apologetic gestures, Galton eventually left the Society in 1905 and went on to establish the Eugenics Education Society in 1907 and a eugenics chair and laboratory at University College London.

The Role of the Relationship between Statistics and Economics in Shaping the Sociological Society's Attitude towards Statistics

An examination of possible opportunities to establish close links between statistics and sociology at the time of the Sociological Society would not be complete without taking into account the contemporary development of statistics and the attitude of the statisticians towards sociology. Some important advances of the British statistical movement have already been mentioned in the introduction; and the attitude of the circle led by Francis Galton and Karl Pearson, has been discussed at length. In this last section, I turn to the close relationship between statistics and economics that characterised the work of the other leading group of statisticians, those at the Royal Statistical Society, and that played a role in the way the statisticians viewed the newly proposed science of sociology.

During the nineteenth century, statistics in Britain developed a close relationship with political economy, later economics, at both an institutional and intellectual level. As already discussed in previous chapters, an important part of the intellectual rationale for founding a statistical section (Section F) of the BAAS in 1833 was the ambition of its founders to reform political economy, with the help of statistics, into an inductive social science (Goldman, 1983). A year later, in 1834, the founders of Section F founded the SSL as part of their ambitious plan. Implementing this plan in practice, however, proved more difficult than initially thought and, as a result, the then London Statistical Society modified their stance towards political economy. From 1834 to 1857 the motto of the SSL, *aliis exterendum,* reflected the Society's intention to co-operate with the political economists as long as the statisticians' work was perceived as distinct and separate, serving only to provide the data to be 'threshed out' by the economists. For two decades this remained the principle on which the Society operated, while in practice their work contained much analysis of the data they were gathering. But once statistics established itself on firmer grounds, the *aliis exterendum* policy became unnecessary and unsustainable – in the 1840s and 1850s, the Society published a few papers arguing in favour of the statisticians involvement in the analysis of the data they were gathering (cf. SSL Annual Addresses 1841, 1843, 1851); in 1856, Section F was renamed, changing from 'Statistics' to 'Economics and Statistics'; and in 1857, the *aliis exterendum* motto was officially abandoned. Thus, after a short period in which statistics made some rather unsuccessful attempts to distance itself from economics, the links between the two subjects were again reinforced. By the 1880s and 1890s, in both Section F and the Royal Statistical Society, the indispensable links between economics and statistics were often explicitly affirmed by the Presidents of both institutions:

> […] the statistical method comes in as an inseparable ally of economic speculation […] Nor have indications been wanting of a desire, still more recently expressed, to break down the wall of division between statistics as generally understood and political economy, and to treat the two, if not as identical, at least as so closely allied, as to be capable of similar or simultaneous consideration (Martin, 1886: 742; 743-4).

> […] the greatest mistakes which economists have made might have been avoided, if they had relied less on a priori reasoning, and had paid greater attention to the facts of human society, which it is the business of Statistical Science to furnish [...] If economic hypothesis is incomplete without statistics, statistics are meaningless without a hypothesis […] If therefore economic science must be incomplete without statistics, statistics are unmeaning and useless unless selected and digested by economic science (Farrer, 1894: 601-2).

This close relationship between statistics and economics was to become formalised within academia – when the London School of Economics was established in 1895, statistics was one of the few main subjects that the School taught and statistics continued to play an indispensable part in the LSE economics degrees throughout the twentieth century (cf. LSE Calendars, 1895-1979).

By the time the Sociological Society was established, therefore, both the statistical researchers working with Galton and Pearson, and those working at the RSS had little incentive for making extra efforts to co-operate with sociologists – the first group had their own distinctive fields of study, namely eugenics and biometrics and their own institution and journal; while the RSS enjoyed powerful links with economics and economics institutions. This is not to say that that statistics and sociology failed to develop a close cooperation *because* statistics already had well developed relationships with other subjects. But, at a time when sociology was looking to establish itself as a distinct and prominent subject, it was made less likely that it would chose statistics as its prime method, given that statistics was already so firmly associated with other subjects and institutions.

However, the *inherent* challenges the Sociological Society faced had a much stronger influence on the course of sociology than the *external* influence of any other social science subjects. The ultimate failure, of a close co-operation at the beginning of the twentieth century between sociology and statistics depended on a number of factors. Among them was the type of sociology the Society wished to promote; the organisational problems within the Society itself, leading to a general lack of positive discussions; the personal motives of its leading members; as well as the position of statistics in relation to other fields of study, such as eugenics, biometrics and economics. Only by examining the interplay of these factors against the background of a well-developed statistical tradition in the UK, can we see that the end result of

the events which took place within the early Sociological Society, was that British sociology was to lose not so much its 'biological roots', as Renwick (2012) has argued; but the opportunity to grow on well prepared and fertile 'statistical' ground. This outcome was *not inevitable*: the combination of factors, examined here, do not add up to a clear and unambiguous anti-statistical position on the part of the sociologists; none of these factors alone would, inevitably, have distanced sociology from statistics at the beginning of the twentieth century when there was nothing inherently a-statistical about sociology and nothing inherently a-sociological about social statistics. It was the chance combination of a number of negative factors, forging the *perception* of an incompatibility that was to determine the subsequent course of events and shape the long-term future of British sociology.

Sociology and Statistics in the Sociological Society: Summary

The Sociological Society set itself a difficult and unprecedented task. In their aspirations, its founders were led by the noble ideal of creating a unified and co-operative community of social scientists who could study society *together*. However, the conclusion of the Society's most active period was marked by the bitter realisation that neither the Society itself, nor the founding of a sociological journal, nor the establishment of academic sociology could make this ideal come true.

In 1911, in an open discussion on 'the things one expected from a Sociological Society', a critic proclaimed that 'indictment' of the Society could be regarded as 'an act of ungraciousness'; but as someone who had waited 'ever since the foundation of the society for elucidation by it of certain problems and month after month met only with disappointment', they thought it necessary to speak out. Their forlorn hopes 'of seeing this synthetic idea emerge' have been 'uniformly disappointed' (Nivedita, 1911: 244-245).

Branford's response to these critiques was ambivalent. He himself had already admitted that although the Society had 'furnished a platform for the presenting of the results of independent investigation' they could not 'pretend to have carried out […] much of Bryce's [the President's] programme' and that this was 'particularly true

with reference to the proposals for co-operative research' (Branford, 1908b). In response to these critiques, he acknowledged that they contained 'much truth and relevance' and even went on to question the whole legacy of the Society:

> Has the Society generated amongst its members the thrill of a common enterprise; or has it merely now and again gathered together manifold representatives for some temporary common end (Branford, 1911: 248)?

But despite his frankly depreciative remarks, Branford was not prepared to hold the Society fully responsible for its own lack of success and instead argued that the problems they had had were due to 'its social milieu':

> Behind the question – What is wrong with sociologists and the Sociological Society? lies the deeper question – What is wrong with society itself? […] Are not indeed those shortcomings of the Sociological Society […] the very characteristics which make our contemporary occidental society so fertile in personal initiative, sectional amelioration and material progress, so sterile in unifying these partial achievements into collective spiritual uplift and concerned social advance? (Branford, 1911: 249).

It is not uncommon for sociologists to use such arguments to explain their problems at a time of crisis[13]. But even if the 'social milieu' was not favourable to the type of sociology it was promoting, the Society's internal problems, as I have shown here, undoubtedly, played a major role in the Society's lack of success and its ultimate fate.

Thus, after only a couple of vibrant and eventful years, the Society led a quiet and marginal existence for the next few decades. During the 1910s it continued to hold meetings and organise various study groups and lecture seminars (Institute of Sociology, 1935). In 1920, the Society was incorporated into a bigger organisation called the Le Play House, together with the Civic Education League and the Regional Association. The Le Play House organised two conferences, one in 1922 on the 'Correlation of the Social Sciences' and one on 'Living Religions within the Empire' in 1924 (Institute of Sociology, 1935). During this period the Society's activities were almost exclusively confined to the civics approach to sociology. Eventually, in

[13] See, Abrams (1981) for a similar explanation of the 'crisis' that British sociology experienced in the 1980s.

1930, the Society with its associated bodies was incorporated into 'The Institute of Sociology' which was dissolved in 1955.

Why should the legacy of the Sociological Society be important, if its struggle to establish sociology in Britain resulted in disorganisation, fragmented scholarship and unrealised ideals? The course of development of British sociology during and after the most active years of the Society, was not inevitable – it was not entirely pre-conditioned by the existence of other social science organisations preceding the Society, as Abrams argued; nor could it have been a direct product of the 'social milieu', as Branford maintained. It was the events, and, more importantly, the *choices* made by the Society, that played the major role. It was these choices – choices about a philosophical ideal on which to base sociology; about a definition of sociology; about a Professorship and a Journal; about an organisational strategy; about the position it took towards the statistical tradition of social enquiry – that had more influence on the course of sociology in Britain than anything that the Society ever did to act upon these choices. Once sociology was associated with a particular choice of words, of thought, of institutions established by the Society, *regardless of whether these choices bore fruit or not*, it was very difficult for sociology to re-organise itself into something different. It took another fifty-sixty years before a different group of people could take advantage of the radically changing social and political environment in order to make fresh choices about what sociology would be, how it would be studied and about its role in society.

Chapter Nine

Academic Sociology and Statistics, 1910-1930

Introduction

As the most active period of the Sociological Society came to an end in 1907-8, British sociology entered what has been widely regarded as a dormant and unproductive period. Scholarly investigations into the most pressing social problems, such as social environment, poverty, population decline continued but not as part of what had been established as academic sociology under L. T. Hobhouse or as part of the work of the institutions that descended from the Sociological Society which focused primarily on regional surveys. An ill-defined, but nonetheless tangible, division was emerging and becoming more deeply entrenched between organisations working on empirical projects, including statistically based projects; and those working within a more abstract, philosophically based academic discipline named sociology. Different historians have tackled this issue differently: while some accounts are predominantly descriptive, aiming only to provide a general chronicle of all the work done with respect to the study of society in this period, others have used the historical evidence to evaluate whether British academic sociology was 'a success' or 'a failure'. In the following chapter, I explain why, in my view, judging this period as a success or failure is neither historically justifiable, nor productive; afterwards I move on to explain how the course of development followed by academic sociology in the 1910s-1930s affected its prospects of establishing firm connections with statistics.

In his account of 'empirical sociology in Britain', Kent (1981) describes the interwar period as a time of the expansion and improvement of social surveys, mainly in terms of the elaborations and extensions made in the use of sampling procedures. Kent's account shows that the survey work done in this period was **not** done by researchers working in academic sociology – notable examples of such survey work include the Bowley and Hogg's 1925 study *Has poverty diminished?*;

the replication of Charles Booth study on London life and labour carried out between 1928 and 1934, *New Survey of London Life and Labour*; Rowntree's own 1936 replication of the study on poverty he originally published in 1901; and Caradog Jones' 1934 Merseyside survey.

As Kent's examples suggest, most surveys conducted during this period examined poverty and living conditions. However, what is more peculiar about them is that despite the fact that some of them utilised the new techniques of sampling, they were still very much representative of the social statistical tradition prior to the development of the so-called new English statistics, which began in the late nineteenth century with Galton, Pearson, Yule, Edgeworth etc. who developed statistical testing and incorporated the theory of probability into statistical research. As Kent concludes, in the interwar period the social survey 'remained an essentially practical undertaking, geared to specific questions and problems'; it was not 'concerned with any comprehensive sociological theory, but it did not even attempt to generate, still less test, specific hypotheses' (Kent, 1981: 114). This trend can also be observed in the survey work on social mobility, as Goldthorpe (forthcoming) discusses in great detail.

Although Kent's account briefly mentions the institutional development of sociology in the interwar period, it is other accounts that have shaped our understanding of the history of sociology in this period. Among them is Evans' 1986 account of the development of the regional movement, as represented by the Sociological Society's affiliate institutions, the Le Play house in the 1920s and from 1930, the Institute of Sociology. Despite various initiatives and projects, these institutions did not leave a long-lasting legacy and, according to Evans, with their non-cooperative attitude towards the rest of social science, they contributed even further to the fragmentation of social science in Britain. Among the reasons that led to this outcome, but also to the decline of the LePlay House and the Institute, was the insistence of the organiser and chief executive officer of the Institute, Alexander Farquharson, that social research be carried out by amateurs – according to Evans', the reason 'why Farquharson rejected quantitative techniques was that they could not be carried out effectively by amateurs'; Farquharson's 'commitment to amateurism was greater than his commitment to objective social research' (Evans, 1986: 420).

However, although Evans provides a large amount of evidence to support the argument that Farquharson was a supporter of amateurism, it remains questionable whether his beliefs about quantitative methods were *directly* linked to his beliefs about amateurism. What is more likely is that neither Mr Farquharson, nor his wife, Dorothea Farquharson who was also involved in the work of the Institute of Sociology, considered quantitative methods as something relevant and important to their work – in her speech at the closing-down meeting of the Institute, Mrs Farquharson argued that, as regards the development of sociology during the nineteenth century, 'our best sociology was to be found in the English novel – Defoe, Thackeray, Dickens, Scott, Charles Reade, the Brontës, Jane Austin and George Eliot' (Farquharson, 1957). Furthermore, it is difficult to see how the attitudes in the LePlay house and the Institute would have had a strong influence on academic sociology, especially since after the rejection of Geddes for the Martin White Professorship at the LSE, the LePlay house and the Institute treated academic sociology with 'a certain measure of healthy scepticism' and 'always avoided becoming too closely involved with the Universities' (Evans, 1986: 60-1).

Kent's and Evans' accounts are aimed more at a description and less at an evaluation of the history of the period. However, there have been a few more powerful accounts that have left a strong impression on our understanding of the history of this period precisely because of their attempts to evaluate it. Soffer (1982), for instance, provides a scathing criticism of the development of British sociology in this period, arguing that the subject failed to develop a 'uniquely sociological theory able to explain social events' (Soffer, 1982: 781) that provided 'incisive social criticism', which Soffer takes to be the crucial element that sociology in the USA, France and Germany possessed and which turned sociology in these countries into a successful subject (Soffer, 1982: 767). Instead, British sociologists 'uncritically endorsed existing social tendencies', with Hobhouse and Ginsberg continuing 'the nineteenth-century sociologists' faith in the inevitability of progressive reform' (Soffer, 1982: 781), holding to an 'inadequate explanation of social phenomena' that 'wasted their potential influence' (Soffer, 1982: 796).

By describing the early twentieth century development of sociology in Britain as a 'failure', Soffer lines up with a number of other scholars who have taken this as

a general view or description of British sociology, and social science more broadly, in this period. Like Soffer, Anderson (1968) blames the failure of British sociology to expand on the absence of a strong and adequate sociological theory; while Abrams (1968) blames it more on the failure to establish itself institutionally and on the negative influence that traditions, such as the statistical tradition, had in 'frustrating' its development and the development of sociological theory in particular. More recently, Rocquin (2014) has challenged the widely accepted twentieth-century view that interwar British sociology was 'a failure'; instead, he argues that British sociology has been 'alive, and that its alleged institutional failure in the history of the discipline is more ideological than factual' and that, on the contrary, it has been 'a success' (Rocquin, 2014: 190).

However, to focus on the question whether British sociology was 'a failure' or 'a success' is misguided. An answer to the question whether British sociology failed or not tells us very little about British sociology in this period – the question may be attractive to some historians but, in fact, all it does is push to the forefront the concerns of the historians at the expense of ignoring the questions and problems faced by sociologists of the day. Of course, one major drawback of such accounts lies in the ambiguity of terms such as 'failure' and 'success'; moreover, to determine what constitutes 'success' in interwar British sociology is to bring into the analysis an arbitrary, inevitably presentist, element which only serves to get in the way of a proper understanding of what was going on. Another drawback of the success/failure analytical framework is that there is no way of knowing whether the things that Soffer and Abrams argue were absent from interwar sociology – a sociological theory, an early well-grounded institutional development of sociology (instead of statistical enquiries) etc. – would have produced 'a successful' sociology had they been present.

It is much more fruitful and historically justified to examine the characteristics of the development of sociology in this period, not in terms of failure or success but in terms of the consequences that this development has had for a particular aspect of sociology. Instead of asking whether British sociology in the interwar period has been a success or a failure, we can learn and understand more by focusing on how the development in this period compares with the earlier and later developments –

how they fit together, what remained the same and what was different. To prepare the ground for answering these questions, I now move on to discussing the major developments in academic sociology in the 1910s and 1920s, with special reference to the impact that these developments had on sociology's relationship with statistics.

Debates in the 1910s – Is Sociology a Science?

There is little direct evidence in the early twentieth-century history of British sociology relating to its relationship with statistics. Most of what we know about this relationship, after the active years of the Sociological Society, has to be inferred from the state of sociology itself, or from the intellectual choices made by academic sociologists at the time.

One of the clearest statements that that the proponents of sociology in this period made against the application of statistical methods to sociological enquiries comes from the 1910 presidential address given at the Sociological Society by Frederic Harrison. Harrison was one of the most famous followers of Comte's philosophy in Britain and a major figure in the positivist club in the late nineteenth century and he had little appreciation for the work of social statisticians and political economists. His address is openly hostile towards statistics: following Comte, Harrison argues that sociology is a science concerned with 'the entire series of laws which apply to social phenomena' but neither statistics, nor statistical laws have a place in this 'entire series' (Harrison, 1910: 97). He argued:

> the master term sociology should be limited to what Comte calls the fundamental laws of phenomena, *and not observations of specific, local or temporary phenomena*. I decline to call geology or geography a science and I decline to treat criminal or vital statistics, notes on experiments in cooperation, socialism or penal legislation as sciences, or even special scientific studies. They are certainly not the science of sociology nor are they even branches of sociology (Harrison, 1910: 100, my italics).

He is here calling for a clear separation between statistical enquiries and sociology and for limiting sociology to the realm of abstract general laws about society (although Harrison did not explain what laws he had in mind). He goes on to

suggest that the frivolous association of statistical or empirical work with the name of sociology has been harmful:

> […] those stupid jibes against sociology may be partly excused by the extravagant habit of some sociologists who are wont to dignify with the name of science loose guesses about things debated in Parliament and *even bare statistics* (Harrison, 1910: 102).

Such overt determination to keep statistics separate from sociology is rare in the history of sociology in the early twentieth century; however, by virtue of being a presidential address representative of the Sociological Society, it remains one of the most explicit statements in opposition of any application of statistics in sociological study.

Not everyone, however, shared Harrison's conviction that sociology was a science. The continuation of the debate about the status of sociology long after the most active years of the Sociological Society indicates that the Society did not succeed in settling the issue. In 1914, a statement adamantly opposed to sociology's claims of being a science was published in the *Fortnightly Review*. Paying special attention to the contributions of Herbert Spencer and Benjamin Kidd, the author, H. S. Shelton, rejected claims that sociology had advanced scientifically:

> To discuss other writers would but show the same characteristics, lack of scientific method and precision, lack of coherence and, possibly, extravagant claims. If anyone interested will examine (shall we say) the *Sociological Review*, or any publication professedly dealing with sociology as such, he would quickly discover how little there was to which the term scientific could, by any stretch of the imagination, be applied (Shelton, 1914: 343).

His conclusion was that, 'at the present state of knowledge…sociology is not a science' (Shelton, 1914: 345). However, Shelton does not exclude the possibility of sociology becoming a social *science*; on the contrary, he argues that 'a study of the structure of society and of societies is both possible and actual, and that the organisation of such knowledge is an ever present necessity' (Shelton, 1914: 344). Shelton includes statistics as a part of this social *science*: 'Whenever exact and statistical knowledge is required, the gathering and formulating of such knowledge is a task which comes within the scope of ordinary scientific method' (Shelton, 1914:

344). It seems, however, that those individuals, and later on even whole institutions, who endorsed a view of sociology as a *science* and that included statistics as having a role to play were, almost always, *not* sociologists – this tendency will become even more apparent in the discussion of post-war British sociology.

Hobhouse on General Sociology and Statistics

Aside from contemporary discussions regarding the status of sociology, the clearest evidence we have about the position of sociology towards statistics in the early twentieth century comes from an examination of the work of L. T. Hobhouse, the only professor of sociology during this time. This section looks more closely at Hobhouse's works with a view to examining in greater detail his understanding of sociology and how this affected sociology's relationship with statistics. It also refers to his successor, Morris Ginsberg, whose main contributions will be discussed in the next chapter, but whose views about sociology differed little from those of his mentor, Hobhouse.

Despite Branford's decision to promote Hobhouse for the professorship of sociology at the LSE, based on his hope that Hobhouse would be a suitable character to unite the various strands of the subject, Hobhouse struggled for years to define sociology in such a way as to make clear its subject matter, methodology, academic practice and, more generally, the direction in which it was heading. Hobhouse's discussion on the nature of sociology is dominated by attempts to define sociology on the basis of philosophical principles; but these remain vague, even confused, and show little progress in the definition and practice of sociology in the early interwar period:

> In a wider sense sociology may be taken to cover the whole body of sociological specialisms. In a narrower sense it is itself a specialism, having as its object the discovery of the connecting links between other specialisms (Hobhouse, 1920: 655).
>
> General sociology is neither a separate science, nor it is a mere synthesis of the social sciences. It is a vitalising principle that runs through all social

> investigation…stimulating enquiry, correlating results […] (Hobhouse, quoted in Ginsberg (ed.), 1966: xiii)
>
> Sociology is one of the methods by which the human problem can be studied as a whole (Hobhouse, 1923: 62).
>
> Essentially, the subject-matter of Sociology is the interaction of individual minds, each in a manner cased in its own shell for ever divided from its nearest, yet reaching out to one another, responding and craving response, co-operating willingly and unwillingly, consciously and unconsciously, yet at the same time jostling, thrusting one another aside, tramping down the weaker, with partial aims vividly realised and deeper common needs imperfectly understood, moving in the mass on lines which no foresight of theirs has traced, yet not without eventual power of self-guidance and an emergent vision of the true goal (Hobhouse, 1924: 11).

Hobhouse's definitions prefigure some of the concerns that came to dominate British sociology later on (self and identity, the nature of communication in sociology, power and legitimacy etc) but at the time they failed to take the subject any further. At the time Hobhouse was writing, the *Times Literary Supplement*, complained that sociology 'is a term with a content as vague or arbitrary as you please'; the 'general impression of it […] is like the popular notion of a comet – a monster with an orbit from nowhere to nowhere' (Unknown Author, 1924: 812). And some years later, Branford himself complained that sociology was still:

> a rather perilous word. It is often used, more often abused, and, in some quarters, uniformly boycotted […] or ignored as a pretentious claimant intruding into a field assumed to be covered by studies like economics, political philosophy, jurisprudence, history. Men of business rightly find it a difficult word for a difficult thing (Branford, 1930: 144).

Despite the vagueness and confusion in Hobhouse's definition of sociology, there was nothing in that definition itself – that sociology is a general science – that excluded statistical enquiries from sociology. On the contrary, it will be recalled from what was said earlier, that a few of the writers in the *Sociological Papers*, such as Branford, Durkheim and Webb mention statistics as one of general sociology's specialisms. So the rejection of statistics was not inevitable; instead, lack of engagement with statistics resulted from Hobhouse's (and later Ginsberg's) choice to limit general sociology to philosophical enquiry. It is also significant that Hobhouse's attempts to define sociology do not say anything about sociology's

research questions. Therefore, although neither Hobhouse, nor Ginsberg spoke clearly against the use of statistics in sociological inquiry, the way they conceived of the subject – as an exercise in philosophical analysis, rather than as a *research-oriented* discipline – made it very unlikely that either of them would do anything to establish links between sociology and statistics.

If it was not the definition of sociology itself, then perhaps it was the method that Hobhouse and Ginsberg chose as sociology's main method that made the incorporation of statistics difficult? Hobhouse's and Ginsberg's chosen method was the comparative method, as it was first developed by Comte. The comparative method was based on historical comparisons between different stages in the development of society or societies:

> Our method must be not so much historical as comparative. It must consist in a review of the multifarious forms of human achievement, with a view of scientific classification (Hobhouse, 1911: 111).

There is some degree of difference between the principles on which the comparative and statistical methods operate and between their conceptions of society. The comparative method sees society as something that changes with the passage of time, looking at causal links between past societal forms and present societal forms; examining progress and evolution on a grand-scale. However, the statistical method sees society as something that not only changes in time but also as something that changes *particularly* as a result of the random, unpredictable variations in the actions of its members; it sees society as an *emergent* phenomenon and places emphasis on the interaction at any given point in time (which can later be compared to interaction at another point in time).

But regardless of these differences, there is nothing in the comparative method itself that excludes the possibility of using statistical data and statistical techniques – in principle, both methods could be combined and used alongside each other. The comparative method was modern inasmuch as it rejected the abstract, deductive approach to studying society. But it was not suitable for studying what was going on in a rapidly changing modern society unless it used statistical data and analysis. Hobhouse's and Ginsberg's enquiries, however, were not about the conditions of

modern society *per se*; they focused instead on things such as morals and ethics and grand-scale social evolution in which statistical techniques were not applicable. All this shows that it was not because of their philosophy or method or definition of sociology that Hobhouse and Ginsberg did not initiate a statistical branch of sociology; neither their definition, nor their method were in principle incompatible with statistics. They focused on types of intellectual enquiry – aimed at describing social phenomena on the basis of general principles – for which statistics was irrelevant. But this was a matter of choice; and it was not a choice that was inevitable.

An example showing that the two methods are compatible comes from Hobhouse's and Ginsberg's own work on *The Material Culture and Social Institutions of the Simpler Peoples: an Essay in Correlation* (1915). At first sight, this study may sound fairly statistical but, in fact, it is not and is, indeed, more of an example of a neglect of statistics. According to Fincham (1975: 55) it was Ginsberg who initiated this project: '[Ginsberg] had come to the LSE as a research student in 1910 after graduating in philosophy at King's College, and was working on the Material Culture of the Simpler Peoples'. The study is essentially a comparative anthropological study. Its purpose was to establish if there was an association between material culture in pre-modern [simpler] societies (1) – expressed as a means of obtaining food and divided into three categories Hunters, Pastoral societies and agricultural societies – and social institutions, such as Government and Justice, Family, War and Social Structure (2). Although the book is subtitled 'An Essay in Correlation', the statistical methods employed in it are crude and elementary and do not go beyond the comparison of percentages. At the beginning of the book there is a section entitled *The Possibility of Sociological Correlation* but there is no discussion of statistical methods and the word correlation is used as a non-statistical term. In addition, the methods it used were out-dated – they used techniques developed by the anthropologist Tyler in the 1880s, despite the fact that by 1915, more sophisticated statistical techniques for association and correlation had been developed.

It remains unclear why Ginsberg, Hobhouse and Wheeler embarked on this study using out-dated techniques. Immediately after its publication, the book was criticised for using out-dated methodology in the *Journal of the Royal Statistical Society*:

> the manner in which the [statistical] method is applied cannot, in view of the development of statistical science in recent years, be said to be sufficient. Percentages are used throughout but although this is certainly a means of indicating association it is insufficient since the coefficient of contingency was devised to deal with problems essentially similar in form to those discussed in this book, and it's a matter of regret that this coefficient was not ascertained for at least a few of the most important tables (Snow, 1916: 69).

Another review, this time of the second edition of the book, commented that the book owes its influence 'less to the positive contributions than to its defects'; to errors of method and execution so obtrusive, even in 1915, that they 'fairly shouted for correction' (Murdock, 1966: 261-262). The book was also criticised for making no effort at sampling, which makes the correlations less meaningful. The book, they say, is 'chiefly useful as an object of lesson of methodological error' (Murdock, 1966: 261-262).

It is possible that Hobhouse, Ginsberg and Wheeler did not use modern statistical techniques because they did not know how to handle them. But then the question arises of why didn't they get a colleague to help them? It is also possible that they did not use statistical techniques because they disagreed with the underlying principles of the statistical approach to studying society. There is some evidence of this in Hobhouse's writings, although Hobhouse does not make specific reference to the *Simpler Peoples*. In his view, *society as an existing entity is not measurable*, only individuals are:

> Even if the idiosyncrasies of *individuals* could be in some measure reduced to law by the *fiction* of averages, the life of *a society* to which millions of individuals contribute each in individual ways is so unique that no generalisations apply to it (Hobhouse, 1920: 658; *italics mine*).

According to Hobhouse, therefore, society consists of something which is *beyond* the millions of individuals which compose it and the results of statistics cannot take us to this level. Although statistical data can help in comparative research, for Hobhouse, society is not a statistical phenomenon:

> [...] we may learn a lesson from the faith of Malthusianism. Mathematical arguments drawn from the assumption that human actions proceed with the statistical regularity that might be found *in a flock of sheep* are often exceedingly difficult to refute in detail yet they rest on insecure foundation [...] *man is not merely an animal*; he is also a *rational* being – reacts to new circumstances in a way that can only be determined my taking the possibility of rational purpose into account (Hobhouse, 1911: 15).

According to this, statistics could be better used to study the behaviour of animals and not men. Mind and reason, the rationality with which people are endowed cannot be measured using statistics. All this suggests, at the very least, that for Hobhouse the application of statistical techniques in sociology was a minor concern.

More evidence on Hobhouse's methodological position comes from an argument he had with William Beveridge, who was a director of the LSE from 1919 to 1937. Hobhouse, following Comte and Durkheim, maintained that the social sciences should employ their own methods, which are different from the methods of the natural sciences. While according to Beveridge, the only effective way of studying society is by using the methods employed in the natural sciences. In both his inaugural and farewell addresses, Beveridge argued that 'the methods of all sciences are the same', including observation, comparison and classification, deduction and verification (Beveridge, 1921: 3) and he was later to argue that economics, politics and sociology should be taught and learned 'not from books but from observations, not from the positions of philosophers, but from the conduct of mankind' (Beveridge, 1937: 470). The argument between Hobhouse and Beveridge arose at a time when at the LSE, in the late 1920s, there was already antagonism between the sociology department represented by Hobhouse's social philosophical tradition; the applied tradition of social research in the Social Science and Administration (SSaA) department; and Beveridge's idea of empirical, biologically based social science, which was eventually realised in the form of a Social Biology department (Harris, 1997). Beveridge was convinced that:

> the social sciences were still 'too theoretical, deductive, metaphysical' and that 'the way ahead' lay in empirical studies of social phenomena rather than in deductions based on analytical postulates about the nature of human behaviour (Harris, 1997: 277).

According to Beveridge, neither the sociology nor the SSaA departments followed the principles which Beveridge believed in – Hobhouse frowned upon any attempts to model the social sciences on the biological sciences; whereas the SSaA department produced studies dealing with the alleviation of social problems rather than broad scientific discoveries. What exacerbated the controversy over the nature of the social sciences at the LSE was an attempt by Beveridge to impose his vision on his colleagues as well as his increasing intolerance towards all other kinds of social research. Beveridge insisted:

> To complete the circle of the social sciences, a third group of studies is required, dealing with the natural bases of economics and politics, with the human material and with its physical environment, and forming a bridge between the natural and the social sciences (Beveridge quoted in Harris, 1997: 279).

This was the basis of the new research programme, 'Social biology', designed by Beveridge and eventually funded by the Rockefeller foundation. Hobhouse, however, was critical of the programme and protested that the Sociology department had not been consulted; while privately, Beveridge believed that the tradition represented by Hobhouse "does not necessarily go with social science" (Beveridge quoted in Harris, 1997: 280).

It would be an exaggeration to say that Hobhouse's and Ginsberg's work exerted any major influence on the development of sociology in Britain after the Second World War; in fact their legacy was limited, and they were hardly mentioned by the new generation of post-war sociologists who appeared to have more immediate concerns than Hobhouse's and Ginsberg's abstract philosophical sociology. But the divide between 'general sociology' and statistical investigation that began with the Sociological Society was perpetuated by Hobhouse's and Ginsberg's indifference towards statistical methods, even without a clear opposition.

Statistics, Sociology and Eugenics

Many of the researchers developing the 'new English statistics' were also involved in the study of eugenics, with Francis Galton, Karl Pearson and his colleagues being the most obvious examples. Could it have been the case that the association of the 'new English statistics' with research in eugenics[14] deterred those scholars, who objected to eugenics, from using statistics? Hobhouse was critical of eugenics from the very beginning. At the early meetings of the Sociological Society, he said of Galton's paper on eugenics:

> […] until we have very definite information as to what heredity can do, I think those of us who are only students of sociology and who cannot lay any claim whatever to be biologists ought to keep silence. Both stock and environment are of fundamental importance. Until we have far more knowledge and agreement as to criteria of conscious selection, I fear we cannot, as sociologists, expect to do much for society on these lines (Hobhouse, 1905: 63).

And in *Social Evolution* (1911) Hobhouse argued that explanations of social phenomena should be based on 'the nature of society itself' (Hobhouse, 1911: 17), not on the genetic nature of individuals. Hobhouse therefore opposed the underlying assumption of eugenics – that answers to biological questions are essential to solving social problems and understanding society. He also opposed some of the fundamental assumptions that eugenics made – mainly that it is possible to estimate genetic worth (the good characteristics that must be sustained through artificial selection or eugenics). He correctly pointed out that there was lack of sufficient knowledge as to how 'good character' is genetically produced. And he appears to have recognised that behind many of the claims eugenics was making was the aspiration to maintain class distinctions and the existing social order (Hobhouse, 1911).

It is unclear whether Hobhouse's attitude towards eugenics was the driving force that determined his position towards statistics mainly because his discussions of eugenics do not contain any references to statistics. One thing is clear, however: for Hobhouse, society was about social conceptions, historical change, laws, religion, ethics, rationality; for him social problems were philosophical questions, not material

[14] For an overview, cf. MacKenzie (1979, 1981).

for statistical analysis. In the sociology of Hobhouse, statistics may occasionally be a useful addition, but it was not essential or even desirable.

If the sociologists were not using the new English statistics, then *who was* using it? And was the use of the new statistical techniques limited to research into eugenics? Goldthorpe (forthcoming) suggests that the fact that techniques such as correlation and regression were developed by eugenicists such as Galton and Pearson is not the reason why others, including sociologists, did not use them. Goldthorpe's study, focusing on social mobility research, clearly shows that those whose work would have benefited most from these new and powerful techniques but who, nonetheless, failed to utilise them, were *both* opponents and supporters of eugenics. And, to my knowledge, there is no direct evidence in the literature of any explicit statements of rejection of the new techniques because of a link with eugenics.

One way to probe this question further would be to find who, if not sociologists, was using techniques such as correlation and regression in the period 1880-1940. An indication can be found by examining the articles published in the *Journal of the Royal Statistical Society* and *The Sociological Review* (the two main journals for statistics and sociology in this period).

An online search through the publications in the *Journal of the RSS* in the period 1880-1940 which contain the term correlation in the main text suggests that the technique was applied to study a variety of subjects – topics in the field of economics, such as finance, price indexing, unemployment, national income, control of the quality of products, consumption of alcohol, the cinema industry, epidemiology, meteorology and vital statistics. There is also a vast number of papers which were concerned with the development and improvement of mathematical statistics and theory of statistics – these are predominantly technical papers. A brief look at these papers clearly shows that it would be wrong to exclusively associate the technique of correlation with eugenicist research.

A search with the keyword 'regression' in the same publications in the same period brings fewer results, largely technical papers related to the mathematical development of regression and other closely related statistical techniques. It would appear that regression found less application in substantive disciplines in this period but this may be explained by the fact that there were practical difficulties involved in

conducting a regression analysis at this time that required a great deal of resources to overcome.

We can use a similar search to find out about the engagement with sociology in the *Journal of the RSS*. A search with a keyword 'sociology' in the same pool of articles from the same period as above, brings up a very small number of articles. The majority of these articles contain bibliographical material; while the rest are presidential addresses from Section F of the BAAS, which mention sociology as part of a list of social science subjects. Only one paper published in the *Journal of the RSS* between 1880-1940 actually discusses sociology – Sidgwick's 1885 address in which he severely criticises the whole idea of sociology as a credible science.

An examination of the articles published in *The Sociological Review* between 1908-1952 with the keyword 'correlation' (appearing anywhere in text) brings up 25 results in total; but only three of the articles use correlation in the statistical sense and apply it in the analysis. Two of the articles are by Pearl Moshinsky, who was part of the Social Biology department at the LSE and who examined intelligence and its relation to poverty, fertility, education and social class; the other paper is on business cycles in Australia. A search for 'regression' brings up two results, neither of which involves statistical analysis at all. The only *sociological* journal in Britain was clearly not a venue for statistical research.

This is just a brief search that can only give us a general overview. It nonetheless shows that the use of statistics was spreading to other subjects and that the techniques developed by Pearson were not exclusively associated with eugenics. Sociology found no place whatsoever in the *Journal of the RSS*; neither did the new English statistical analysis based on correlation and regression, find a place in sociology's only journal, *The Sociological Review.*

Sociology and Statistics in Britain, 1903-1930: Summary

The last two chapters covered the tentative beginnings of British academic sociology. Since the years 1903-1907 are a time of the first concentrated efforts to establish sociology on secure grounds, we tend to see this period as a watershed,

regardless of the real outcome of the Sociological Society's efforts. In my account of the history of this period and the interwar decades that followed, I have distanced myself from the discourse about success or failure that pervades the most influential historical accounts previously published. I have argued, instead, that it is much more insightful and historically faithful to examine this period firstly, by putting it into the context of the development of social science in Britain in the nineteenth century and, secondly, by tracing its own meaning as a context for the development of sociology after the Second World War.

With regard to the first point, I have shown that as social science developed in nineteenth-century Britain, it offered two broad traditions from which a sociological society could choose even if they did not necessarily associate themselves with any particular institution: there was the empirical statistical tradition, which offered to provide the scientific potential of studying the social world in a similar way to the natural world; but there was also the Comtean vision of a social science which offered a systematic and coherent, but also non-quantitative, type of sociology. The Sociological Society turned out to be the stage for an interplay of factors that saw British sociology choose a 'Comtean-like' path, rather than the path laid out by the social statisticians. It is more significant that the Society made a series of decisions regarding the choice of path, than how far, or how successfully, they themselves actually walked that path.

With regard to the second point about the importance of the early twentieth-century development of sociology as the context for what followed: the significance of the legacy left behind by the relatively dormant existence of British sociology following the decline of the Sociological Society comes not so much from the fact that their work brought about success or failure; but from the fact that when more favourable social, political and academic conditions for sociology emerged immediately after the Second World War, this prior period of stagnation proved not to have been a handicap but sociology's most valuable asset.

PART THREE

SOCIOLOGY AND STATISTICS IN BRITAIN, 1930-1990

Introduction

As discussed in the previous part, existing literature on the early twentieth-century history of sociology in Britain has been dominated by a discourse about the 'success' or 'failure' of sociology to develop institutionally and intellectually. The historiography of post-war British sociology has taken a similar form, although the focus of the debate has shifted from discussions of whether the development of British sociology has failed or not to whether the spurt of development that did take place was *exceptionally* late, or not.

Thus there are scholars who argue that the nineteenth-century development of social, but not sociological, studies, such as statistics, social reformism and social administration, resulted in the failure of a 'proper' sociology to emerge in Britain. These scholars also argue that the establishment of the first sociological institution, the Sociological Society in 1903, and the academic expansion of sociology in the post-war years, signifies an exceptionally late arrival for sociology in Britain (cf. Annan, 1959; Abrams, 1968; Anderson, 1968; Soffer, 1982).

In contrast, other scholars focus on what they see as 'successful' steps in the development of social science and sociology in Britain and tend to argue that the social science movements that existed before the institutional establishment of sociology at the beginning of the twentieth century should not be excluded from the history of sociology or seen as hindrances to its development (Goldman, 1987, 2002; Kent, 1981; Kumar, 2001). They also argue that the institutional development of sociology in Britain should not be considered 'exceptional' when compared to other countries and that British sociology's 'arrival' in academia and its intellectual take-off in the post-war period cannot be characterised as unusually late.

Regardless of the differences in their interpretations, both groups of scholars have been obsessed with the 'late' arrival of British sociology, one arguing that it came 'exceptionally late', the other arguing that it is not unusually late; and both groups have been obsessed with the contrast between British sociology's dormant early twentieth century existence and its post-war boom – either to justify this contrast as normal, or condemn it as exceptional.

Consequently, other, potentially more illuminating, approaches to understanding the history of twentieth-century British sociology have been obscured in the historiography. One such neglected approach focuses on explaining how the social philosophical, non-quantitative tradition to which the Sociological Society and its affiliates adhered, could exist parallel to, but separate from, the more empirically oriented and statistical traditions of social enquiry. In Bulmer's words: 'almost all of the empirical social research undertaken in Britain between the wars went untouched by what then passed for academic sociology' (Bulmer, 1985: 4). He has further emphasised that the difficulty in understanding these two parallel traditions is not resolved by treating one as a subset of the other and that we can achieve deeper and better understanding only by acknowledging the division and analysing what sustained it – something that has rarely been done.

In this part, it is my intention to find out to what extent this divide persisted when other people and institutions took over where the Sociological Society and its affiliates left off and with what consequences for the relationship between sociology and statistics. I will examine how this divide was maintained in the rapidly changing academic environment that characterised the early post-war period; and why academic sociology *could* expand while remaining separate from the empirical tradition of social enquiry.

Chapter Ten

Intellectual Debates in Sociology and Statistics in Britain, 1930-1970

Introduction

The history of sociology in the 1930s and the early post-war period was rich in discussion about the character of sociology, its academic future and its methodological development. The aim of most of these discussions was to find the best way to turn sociology into a subject that was both academically respected and 'scientific' (although the precise meaning of the word scientific varied among sociologists) and to close what was perceived to be an existing gap between sociological theory and research methods. The question of the role of quantitative methods in sociology was also widely discussed.

This chapter analyses the major intellectual shifts and methodological concerns that characterise the period between 1930-1970. It surveys the intellectual development of sociology in this period and assesses how the changes that occurred influenced British sociology's relationship with the empirical tradition of social enquiry and, more specifically, with statistics. The chapter pays special attention as to how sociologists reasoned about and justified their choices about the type of sociology they wanted to see established in Britain, especially after 1945.

British Sociology in the 1930s

In the 1930s, there was a widespread lack of clarity about what sociology was and what it aimed to achieve intellectually and academically. This period saw a number of attempts to redress this situation, most notably by Morris Ginsberg who became the Martin White professor of sociology at the LSE after L. T. Hobhouse passed away in 1929. Ginsberg, as well as other prominent individuals engaged with

social science, also made an effort to establish sociology at the BAAS and worked to set up a social science research council. Both attempts failed. A focused discussion on the methodology of sociology had not yet begun, although some of Ginsberg's writings make it clear that incorporating a statistical worldview and quantitative methods should not be a primary aim for British sociology.

As a professor in sociology at the LSE in 1930s, Morris Ginsberg wrote a number of books and articles in which he made an attempt to clarify the subject matter and methodological approach of sociology. Ginsberg's *Studies in Sociology* (1932), *Sociology* (1934) and his later articles (Ginsberg, 1937; 1939) contain repeated attempts to define sociology; however, rather than making it easier and clearer to understand what sociology is, Ginsberg's writings tend to leave the reader confused about the subject. For instance, in the scope of just a few pages, Ginsberg argued that sociology was 'the study of human interactions, their conditions and consequences' (Ginsberg, 1932: 1); that sociology was 'the science which deals with social life as a whole in contradistinction from the special sciences' but, also and somewhat contradictory, that it was not 'more comprehensive' than the special sciences, but dealt with their topics 'from a special aspect' (Ginsberg, 1932: 3, 7). Only to add to the confusion, Ginsberg then admitted that he doubted whether sociology 'can ever be an independent science' (Ginsberg, 1932: 8). His other works on sociology are no less convoluting and disorienting.

Ginsberg's position toward quantification was similarly ambiguous. His macro-sociological approach to sociology did not totally exclude empirical or quantitative study from sociology: one of the objects of sociology was 'to formulate empirical generalisations' (Ginsberg, 1932: 16) by studying 'empirical associations, or correlations, of varying degrees of difference between concrete social phenomena' (Ginsberg, 1939: 472). In a discussion of the specialist social studies that are to be part of a general sociology, he mentioned 'social statistics as continued by the successors of Quetelet' (Ginsberg, n.d.: 3). He also argued that any conclusions that sociology reaches always had to be 'tested by appeal to the concrete facts of social life' (Ginsberg, 1932: 8) and that the comparative method was partly based on the quantitative measurement of the interrelations between societal institutions (Ginsberg, 1932: 16). But, in spite of all this, for Ginsberg, the comparative method was the

sociological method *par excellence* – Ginsberg was clear that statistical studies are only suitable when sociologists study 'without going outside [a particular] society or period', a limit which was unthinkable for sociology as Ginsberg saw it (Ginsberg, 1939: 476). The role of statistics was also limited to providing description – as soon as sociology moved from the descriptive to the analytic level, it was the comparative method that was 'essential' (Ginsberg, 1939: 476). And last, but not least, factual data, statistical or not, could not possibly be placed at the centre of sociology because it had no bearing on ethical issues, such as values, which were crucial for sociology.

Thus Ginsberg would often make depreciative remarks about quantification, protesting that 'while students are given careful instruction in marshalling and correlating factual data [which, in the LSE's case could only have been true in the subject of economics], they have no parallel experience in weighing values' (Ginsberg, 1937: 322). Two decades later, Ginsberg would still be arguing that 'sociology has most to gain from a comparative study of civilisation' and that it should be closest with comparative law, comparative religion and morals and the comparative study of economic institutions (Ginsberg, 1956: xiii). And although Ginsberg's own experience in working with quantitative methods – his 1915 study with Hobhouse and his 1920s study on social mobility – have been posthumously praised by MacRae (who had little quantitative knowledge himself) (cf. Marshall and MacRae, 1970: 360), both studies have been heavily criticised by quantitatively skilled social scientists (cf. Snow, 1916; Murdock, 1966; Goldthorpe, forthcoming). However, even if Ginsberg had 'a keen sense of the factual and was capable of making shrewd remarks about the reliability of the observations reported by X and Y' (Freedman in Fletcher, 1974: 270), Ginsberg rarely applied these abilities, if he indeed he had them, in his sociological practice and did little to encourage their development in students.

As with his predecessor L. T. Hobhouse, Ginsberg made no contribution to foster an appreciation of quantitative methods in sociology and nourish the development of such skills among his readers or students during his time in the Martin White Chair. And, similarly to Hobhouse, Ginsberg made no attempt to address the parallel but separate existence of an empirical tradition of social enquiry within other departments of the LSE, such as the Department of Social Science and

Administration and the short-lived department of Social Biology. There is no evidence that either Hobhouse or Ginsberg saw the divide between the two traditions of studying social matters as problematic. A shift in this attitude occurred only after the Second World War.

Commemorations of Ginsberg's work tend to emphasise that the direction in which British sociology was moving from the 60s onwards was at odds with Ginsberg's own vision; as a result of which, he very quickly came to be perceived, even caricatured, as an 'old-fashioned' and 'outmoded' figure who was 'redolent of nineteenth-century sociology and philosophy' (Fletcher, 1974: 1) and leading him to being written off 'as a non-entity' by his younger colleagues (Albrow, 1986: 338). But one aspect of Ginsberg's views that remained unquestioned throughout the post-war period was his ambiguous position and overall scepticism towards quantification. It is reasonable to argue that although post-war sociology moved away from Ginsberg's ideal for sociology, it retained much of his passive and sceptical attitude towards quantification, more generally, and statistics, specifically. Even when he was seen as old-fashioned, aspects of Ginsberg's approach remained a source of admiration in the 70s when dissatisfaction with quantitative methods was growing – 'It seems to me that Ginsberg's greatest contribution was to preserve the tradition of the study of ideas and principles of the things which cannot be exactly calculated' (Caine, 1974: 31-32). Therefore, even if Ginsberg did not openly oppose empirical quantitative research, he did little, if anything, to promote its development *within* sociology because he did not believe it was essential.

Ginsberg's efforts to define sociology were perceived from the outside as questionable, at the very best, and unsuccessful, at the very worst. It could be argued that this made it more difficult for quantitative social scientists from outside of sociology to consider engaging with sociology. In 1936, there was an attempt to establish a sociological section at the BAAS. The council minutes of the BAAS for 6th June 1936 contain reference to a correspondence regarding the possibility of establishing a sociological section or subsection in the Association; but the council decided that this was unnecessary because sociological topics could be discussed in some of the already existing sections, like Section F (Collins, 1978: 133). It was not clear how a sociological section would enhance debates within the Association and,

when Ginsberg approached the Association in 1937 with a proposal to change the name of Section F (Economics and Statistics) in order to make possible the inclusion of papers in social science[15], his proposal was also turned down. Allowing for some prejudice on the part of the Association against sociology, which is not made explicit in their writing but was common in the 1930s, 'the extent of the British Association's real commitment to the social sciences is difficult to pinpoint' (cf. Collins, 1978: 133, 147). It is also likely that Ginsberg's own writings, with their highly abstract and contradictory statements, played a part in the failure to convince the Association. When a sociological 'Section N' was finally established in 1960, this was mainly as a result of the academic popularity of the subject at that time and not necessarily because sociology had made clearer its subject matter and methodology (see next chapter for more details).

Another sign of the difficulties that sociology was encountering in the 1930s was the lack of success in establishing a social science research council. Sociology was in a precarious position not only because it lacked a clear sense of direction but because social science was met, more generally, with suspicion and hostility: G. E. G. Catlin, one of the first supporters of a Social Science Research Council (SSRC), for instance, recalled that while 'the social sciences have been regarded as wicked attacks on the humanities by the humanists', they have also 'been regarded as bastard little brothers by the physical and natural scientists' (Catlin, 1942: 88). So; when in 1930, Catlin obtained financial aid from the Hally Stewart Trust in order to set up a committee 'to survey the social research work being carried out in Great Britain' and decide on whether establishing a social science research council should be recommended (Catlin, 1942: 89), the prospects did not look very promising. William Beveridge, Alexander Carr-Saunders and Alexander Farquharson, among others, were on the committee. They reached the conclusion that a council was necessary and should be recommended. For reasons that remain unexplained, however, 'the broader scheme [of establishing a council] was abortive' and, partly due to the activity of Lord Stamp, a committee member, an Institute of Economic and Social Research was established instead. The original proposal was revived during the war

[15] An unsuccessful attempt that very much resembles John Ingram's 1878 proposals for reforming Section F into a 'sociological section' (see Part One). This similarity suggests that little has changed between sociology and the BAAS between 1878 and the 1930s.

and a new committee, which included Carr-Saunders, Sargent Florence and Morris Ginsberg in the Chair, was set up to reconsider it. This again proved unsuccessful, with Catlin and Robinson listing the following as main reasons:

> (a) that the Vice Chancellors' Conference could handle the matter better;
> (b) that we didn't want any Americanisation of British Academic Habits;
> (c) that the British tradition was, not to channel funds under a committee, but to encourage the individual scholar to go his individual way […] (Catlin and Robinson, 1962: 13).

The reasons pointed out by Catlin and Robinson speak volumes about the climate in which Ginsberg, the leading sociologist in the country, struggled to thrive; throwing light on the difficulties sociology faced in trying to get itself established within academia. As Catlin and Robinson put it in 1962: 'In this country, there being more xenophobia than common market, it appears to take 30 years to get an idea across' (Catlin and Robinson, 1962: 13). One of the main ideas which was proving difficult to 'get across' was the idea of sociology itself.

The process of establishing a SSRC, which began in the 1930s went through three unsuccessful proposals – the first two described above plus the Clapham committee of 1946 which recommended ear-marked grants for social science, but no research council. It was only in 1965 with Lord Heyworth's positive recommendation, that a SSRC was established. As with the establishment of a sociological section at the BAAS in the 1960s, however, the setting up of a research council was more indicative of a profound shift in the general attitude towards social science from which sociology was benefiting, mainly manifested in an increasing student demand to study social science subjects; and not necessarily indicative of an advance which was internal to sociology.

British Sociology in the Early Post-war Period

This shift in the general attitude towards social science, and particularly sociology, manifested itself most clearly in the 60s but it had begun to gain momentum much earlier. Soon after the end of the Second World War, universities

and the public began thinking more seriously about the benefits of social scientific knowledge and of using that knowledge to make sense of twentieth-century society and the problems it faced (the Clapham report of 1946 into the state of social science education was a clear manifestation of that). Despite unquestionable progress in the natural sciences, advances in natural science had also made possible more efficient and larger scale methods of human destruction as in the Holocaust and the atomic bomb. However, since there was nothing inherent in natural scientific discoveries that determined how they were going to be used, the events of the Second World War triggered a strong concern about the mistakes that *society* had made in *applying* natural scientific knowledge: 'If our social skills had advanced step by step with our technical skills, there would not have been another European war' (Survey, 1947: 254); moreover, 'the disaster from which the European civilisation has barely emerged' was seen as 'a call to man to study himself even more than nature, to put economics before engineering, politics before physics' (Beveridge, 1946: 236).

But science was not entirely seen as a source of evil. For some influential social scientists, such as William Beveridge, scientific work relating to the natural world was not enough on its own for human progress; what was needed was more scientific work on society. According to Beveridge, the war experience had made it clear that in order to progress the study of society should actually follow in the steps of Science, not Art, and do away with abstract deduction as its basis and replace it with observation (Beveridge, 1946: 237). Beveridge insisted that, in order to achieve this, the social sciences should be taught together, not separately – an idea which resonated well with Ginsberg's understanding of sociology – but the role of sociology and its ability to help in this process was not discussed by Beveridge. It was argued that although the study of sociology as it had developed in Britain appeared to share some of the ideals that were becoming increasingly popular at the universities after 1945, the universities were 'reluctant to move ahead', most likely because sociologists so far had 'emphasised economic, political and social theory and philosophy to the neglect of empirical studies' (McConnell, 1948: 657). Sociology, in its aspirations and self-understanding, was still out of tune with the aspirations of social science in Britain more generally.

This is perhaps why in the late 40s there was a growing sense among sociologists themselves that sociology should take a clear stance on the empirical tradition; something that, as I mentioned, Hobhouse and Ginsberg had failed to do earlier. How should the *separate* existence of this tradition be interpreted – as a troubling situation exacerbated by sociology's neglectful attitude towards it or, given the intellectual aims of sociology, as something inevitable and normal; as a problem or as an irrelevant issue?

One of Britain's leading social scientists of the 40s and 50s, T. H. Marshall understood the situation as creating a dilemma for sociology – now that the social and political climate was changing and new academic opportunities were likely to emerge for social science, it was necessary for sociology to decide on a suitable road ahead. Marshall saw a crossroads, with two possible directions for sociology: firstly, the road towards abstract theory and the discovery of 'universal laws' and 'ultimate values' – the road 'to the stars'; and, secondly, the road towards empiricism that was most likely to lead 'to the expenditure of great energy on the collection of a multitude of facts with sometimes an inadequate sense of purpose' – the road 'into the sands' (Marshall, 1946: 14-15). Both roads were challenging: on the one hand, efforts to develop a universal theory of society had led many sociologists to perceive of 'a conceptual analysis of society as if it were a substitute for research' (Burgess quoted in Marshall, 1946: 13); on the other hand, efforts to 'bring the social sciences more into line with the natural sciences by making greater use of quantitative methods' had already led many sociologists to slip into a state of mind from which they failed to distinguish between situations where measurement is used because 'the relevant data can be measured' and situations where they measured everything, indiscriminately, regardless of whether what is deemed measurable is, indeed, worth measuring (Marshall, 1946: 15). Marshall recommended 'a middle road' but fell short of a clear strategy on how to find this road and make it a practical reality. Overall, however, Marshall's interpretation can be seen as recognition, perhaps the first recognition by a sociologist, that the separate existence of an empirical (quantitative) tradition was *sociology's concern.* However, it must also be emphasised that Marshall's vision was an anticipation of a crossroads to come, rather

than the crossroads at which British sociology actually stood in 1946[16]. If we were to substitute Marshall's metaphor with an historically more accurate one, we could say that British sociology was standing not before a clearly defined crossroads; but rather, in front of a wide empty field with renewed hopes of the potential of that field to bring a richer harvest than the one that the Sociological Society, or Hobhouse, or Ginsberg had yielded in the previous decades.

This was all the more obvious from the literature on sociology and other closely related social sciences in the late 1930s, early 1940s. In what is regarded as one of the first British textbooks of social science of this period, F. C. Bartlett et al (1939) pinpointed the major problem that the social sciences were facing:

> Everyone pays lip service to the vital necessity for a vigorous development of 'social science'. Yet when ardent investigators, not satisfied by general exhortation and advice ask 'What shall we do? and 'How shall we do it?' few serious attempts are being made to answer them. This lack of detailed guidance is perhaps least marked in economics and in the field of vital statistics; it is certainly most marked in the specifically human sciences of psychology, social anthropology and sociology. Nowhere have these three sciences been properly mobilised to deal with the social problems which yearly grow more pressing (Bartlett et al, 1939: vii).

This was the contemporary view of the leading social scientists in Britain. A further indication of the uncertain position, particularly of sociology, at this time is the fact that most papers on sociology in the textbook were written not by sociologists but by specialists in, for instance, social science and administration, experimental psychology and industrial psychology. The only contribution made by a professed sociologist was by Morris Ginsberg, and he, too, lamented the position of sociology:

> Probably a great deal of the opposition shown towards sociology as a branch of learning is due to the fact that for the philosophers it is not philosophical enough and for the scientifically empirically minded scientists not scientific enough (Ginsberg, 1939: 438).

[16] However, these were the crossroads at which *American* sociology was already standing in the mid 40s and it is not a coincidence that to back up his arguments, Marshall relied extensively on material from the *American Journal of Sociology*, an early sign that the understanding of the American development of sociology was too easily imposed on the British case.

Similar comments came from Marshall himself who admitted: 'sociology has not enjoyed too good a reputation in this country and that even now it is still regarded in some quarters with a certain amount of suspicion' (Marshall, 1946: 4). Tom Harrisson, another shrewd observer of the state of sociology, commented that 'much confusion has centred around the term 'social science' and especially its offspring 'sociology' – sometimes used to cover anything from socialism to social work' (Harrisson, 1947: 10). In another, later attempt at explaining the opposition to sociology, Bottomore argued that the 'opposition to sociology in its early phase came largely from the feeling that it aimed not at co-ordinating but at absorbing the other social sciences' (Bottomore, 1962: 17). As time passed, it was becoming increasingly clearer that pursuing either course – co-ordination or absorption of other sciences – was not a feasible intellectual pursuit.

The picture of sociology as seen from outside of academia in the 1940s was not much different. The Clapham report of 1946, the first in a series of government reports on the social sciences produced in the post-war period, also remarked upon the ambiguous status of sociology. A running theme through the report is that the social sciences are of a 'great practical value' (Para. 3), however it appeared that these sciences, perhaps excluding economics, still had not reached their full scientific potential. As regards sociology, the report refers to an anonymous comment by a holder of a Chair in Sociology[17], according to whom 'sociologists had not yet done the work that they ought to have done to make theirs a fundamental study' (Para. 17). Things changed little in the early 50s, when, according to some sources, the general public still saw as a distinguishing feature of sociology 'the cocksureness rather than the intellectual vitality, the exclusive self-righteousness rather than the pioneering spirit' (Unknown Author, 1954: 727). British Sociology was suffering from 'the collapse of language into jargon' and from giving the layman 'a feeling of being got at or spied on'; the subject was pressingly inviting its own 'doom of ridicule and dislike' (Unknown Author, 1954: 727).

The American views of British sociology were similar. The American sociologist, Edward Shils, who came over to Britain in the 1940s, admitted that

[17] This comment must have been made by Ginsberg, as in 1946 there was no other chair in Sociology except for the Martin White Chair who was at the time occupied by Ginsberg.

although some effort was being made in British sociology towards the incorporation of 'systematic empirical analysis of contemporary society at undergraduate level', in the late 40s there was much 'justified scepticism about the usefulness of research/practical capacities of persons who have had the training in Sociology' (Shils, 1948: 590). What is most curious, however, is that even those sociologists, who were aware of the most pressing problems that sociology was facing, excluded the possibility that developing a statistical side to British sociology could help alleviate these problems. Shils for instance argued that 'an orientation towards immediate practical problems and an enthusiasm for field work and statistics' will not alone suffice 'to enable sociology to overcome its remoteness from reality' and its 'fragmentary and scattered concreteness' (Shils, 1948: 592). A sense of passive acceptance was beginning to form and harden that the alleviation of British sociology's problems was nowhere to be found. Shils' comments with regard to statistics, more specifically, mark the emergence of an attitude towards statistics that would become increasingly prominent in British sociological circles – not yet explicit hostility but a sense that statistical methods are somewhat irrelevant to solving sociology's methodological problems mixed with caution about the damage they can do to the subject.

British Sociologists at a Crossroads, 1945-1965

The intellectual make-up of British sociology which Shils encountered in the late 40s was soon about to change, as new fields of research developed. The change occurred in such a fashion 'as to appear as a sharp break with the past' and was reflected primarily in a change in the 'type of mind' which was attracted to sociology in the post-war period as compared to the interwar period (Banks, 1963: 47). Although the development of sociological theory remained a priority, sociology was no longer dominated by 'men of primarily philosophical bent', such as Hobhouse or Ginsberg; according to Banks, sociology in the early post-war period was beginning to attract an increasingly growing number of 'empirically based investigators whose

interests are set in the framework of scientific rather than philosophical speculation' (Banks, 1963: 47).

Banks' understanding of the change of direction in British sociology is supported by the conclusions of a series of surveys of sociological research from the late 40s, 50s and 60s conducted by Madge (1957), Little (1963) and Krausz (1969). These surveys show that broad evolutionary studies were gradually giving way to more precise and narrowly confined investigations in numerous sociological fields of enquiry such as education, social mobility, social stratification and social class. The change is also clearly reflected in the changes of the sociology syllabus at the LSE (see Chapter Twelve for more details).

This shift was a sign that during the 50s and early 60s British sociology was, indeed, approaching the crossroads that T. H. Marshall was talking about in 1946. It was gradually being recognised that sociology *could* be extended to other fields of empirical enquiry, outside of Hobhouse's and Ginsberg's social philosophy even though there was not one organised way of doing it. More important than the fact that British sociology was approaching a crossroads between the road to 'the stars' and the road to 'the sands', was what was happening on its approach – did sociologists find one common middle road, or did they merely split in two groups carrying on along two separate roads? An effective way to find out is to consider the careers of some of the few, more prominent, social scientists and sociologists in the 1940s-1960s, taking into account their views on the intellectual and methodological make-up of sociology and their mutual collegial relationships. The following analysis takes four such individuals, whose views can be taken as a clear indication of what was about to happen to British sociology when it reached Marshall's crossroads and what implications this had for the possible incorporation into sociology of statistical methods and ways of thinking.

Tom Harrisson

Tom Harrisson is most famous as one of the organisers of *Mass Observation*. In 1947, Harrisson published an article called *The Future of Sociology – What is*

Sociology? In this article, Harrisson defined the state of sociology in a very similar way to Marshall, arguing that sociology was standing at the bottom of two roads, both leading to extreme positions, with empty ground in between[18]. On the one hand, Harrisson argued, the 'philosophical approach to sociology, in which great laws of human behaviour are produced without observation' still exercised 'a heavy high level influence on university curricula and research training' (Harrisson, 1947: 10). On the other, there had been 'vigorous' attempts 'to make the subject as objective and scientific as possible' which in turn had led to 'an absorption of quantitative methods which, with maximum impersonality, satisfy purely 'mathematical' criteria' (Harrisson, 1947: 10). The result was that there was 'a gulf' 'between the extremes of philosophical, subjective sociology, and statistical, quantitative sociology' (Harrisson, 1947: 10).

To fill this gap, Harrisson recommended for sociology what is today known as a qualitative methodology based on 'the intensive study of basic human problems' (Harrisson, 1947: 15) and on collecting what was regarded as real 'human material' – 'stuff of ordinary living' and 'descriptive analysis of people', all 'fascinating and illuminating without questionnaire, sample or formal interview' (Harrisson, 1947: 12). Harrisson was even reported to have argued that 'the social investigator's most neglected piece of equipment was a pair of ear plugs' (Willmott, 1962: 341).

Harrisson was much more explicit about sociology's future than Marshall but they both were heading in a similar direction. Unlike Marshall, however, Harrisson was a vigorous opponent of quantification and statistics as applied to sociology. Following the American sociologists Howard Becker and Peter Sorokin, Harrisson insisted that "sociological problems are not quantitative problems at all" and that social scientists, educators and sociologists 'found in measurement and computation a substitute for real thought' (Harrisson, 1947: 20).

In an attempt to criticise statistics, Harrisson argued that the results obtained through a questionnaire administered to a random sample of Grimsby fishermen would be just as subjective as the results obtained via any other method (Harrisson, 1947: 19). Furthermore, according to Harrisson, surveys could never produce

[18] It would appear that just like Marshall, Harrisson's views were grounded on impressions of American sociology rather than on knowledge of the historical development of British academic sociology.

representative knowledge because they can only say something about social attitudes, which respondents express verbally; they could not say anything about the actual physical behaviour of the respondents. Harrisson was convinced that quantitative research, obscured what was *really* going on in society – 'Among thousands of figures, there nowhere appears the figure of a man' (Harrisson, 1947: 11) In addition, he argued that the knowledge gained from a survey was bound to be misleading because the respondents are prone to lie in their responses. Harrisson's views are echoed in more recent comments made by John Wakeford:

> The quality of what goes in statistics is usually extremely suspect. What was the response rate? In their responses, did people tell the truth? Why should they bother to tell the truth - they are trying to impress you! (Author's Interview with John Wakeford, 2017).

However, to argue that surveys are not representative because they cannot help us understand what is *really* going on in society is to misunderstand the meaning of representativeness in statistical sampling[19]. But also, and more importantly, Harrisson seems to be arguing that there *is* a method that *can* tell us what is *really* going on in society. However, no one can reasonably claim to have special, privileged access to people's private inner world, what is 'really' happening to people (which is what Harrisson appears to be attempting to do by advocating observation of physical behaviour); this would be absurd given the fact that no one, not even people themselves, let alone sociologists, have any way of *proving* the existence of their inner world (for more on the so-called 'the scandal of solipsism' and the development of a similar to Harrisson's approach in the ethnomethodological tradition, see Gellner, 1979). While it does not make sense to put forward claims that some methods allow sociologists to see what is *really* going on in society and others do not; it does make sense to argue that some methods are better instruments for studying some aspects of society than others. The historical question then is why would Harrisson be so adamantly critical towards quantitative methods? Why didn't

[19] In the statistical sense, the results from an analysis of a sample drawn from a population are representative if they reflect adequately the characteristics of the population from which the sample is drawn. How representative the results from the analysis of a sample are depends, above all, on how skilfully and adequately the sample has been drawn from a population and whether it has been analysed using the appropriate techniques – and not, as Harrisson's argues, on whether surveys tell about social attitudes as opposed to physical behaviour.

he make the case about the importance of the use of qualitative methods as complementing the knowledge obtained through quantitative methods and, instead, present them as two necessarily extreme and mutually incompatible approaches?

The answer, of course, is that while Harrisson *could* present quantitative and qualitative methods as complete opposites, *there was no need for him to do so*; in other words, there is nothing inherently incompatible about quantitative and qualitative methodology. The importance of Harrisson's article lies, therefore, in the *choices* of interpretation he makes rather than in whether he supports his interpretations with sufficient evidence.

Firstly, like Marshall before him, he chose to portray the future of sociology as two distinct possibilities, two separate roads – had Harrisson, or even Marshall, attempted to produce historical evidence that British sociology had already been 'contaminated' by a 'quantitative obsession' which enjoyed 'undue dominance' (Harrisson, 1947: 24), they would have struggled because, as this study has shown, this had never been the case in Britain. Harrisson's interpretation is important for introducing into the more or less empty field of British sociology in 1947 the influential, but mythical, idea that British sociology needed to find a way *out of a quantitative obsession* that it never had experienced in the first place.

Secondly – by choosing to portray quantitative and qualitative methods as incompatible opposites, Harrisson was setting a precedent in British sociology which would be followed to an extreme later, in the 1970s, during the so-called positivist disputes. It is with Harrisson that this kind of extremist rhetoric about methods – qualitative *versus* quantitative – really began, which had the effect of limiting the possibilities for a balanced and practically oriented approach based on effective communication between social scientists skilled in different methods.

Finally, Harrisson's essay had important implications with regard to attitudes towards quantification. He set another precedent, which would develop into a trend in the later post-war history of British sociology, of dismissing statistical methods out of hand, *without* having the requisite know-how to make a competent judgment about these methods and their proper use. An important part of this trend would be, just as in Harrisson's case, focusing almost entirely on the damage that

indiscriminate quantification may inflict upon sociology, without taking into account any potential benefits.

Harrisson's writings are a clear indication that finding a middle road for sociology was increasingly becoming a remote possibility. A more likely scenario was that sociologists would split into two groups – those like Harrisson and, at least to some extent, Marshall, who may have acknowledged in principle that statistics may be important, generally, but advocated a non-quantitative future for sociology; and those for whom sociology could not be without quantification. This is well illustrated in the case of three prominent and quantitatively minded social scientists who ended up on the fringes of sociology despite their commitment to the subject; the first one of them being Barbara Wootton.

Barbara Wootton

T. H. Marshall's 1946 lecture on 'Sociology at the Crossroads' was first delivered as an inaugural address when Marshall became a professor in Social Institutions at the LSE in 1944. Competing against him for the post was Barbara Wootton (1897-1988), an economist by training, who, by the mid-forties, had widened her academic interests to include the broader spectrum of social science. In a recent biography of Wootton, Ann Oakley describes in detail the 1944 competition for the LSE professorship, concluding that 'had Barbara got the LSE post, the future of sociology at the LSE might have been quite different' (Oakley, 2011: 190). This is not an unreasonable suggestion, given that Marshall and Wootton differed significantly in their social scientific outlook even though neither of them held extreme views about what was the 'right' path for social science. As Oakley puts it, Marshall was 'the man with a theoretical disposition', while Wootton was 'the woman whose mind turned on the empirical usefulness of the social sciences' (Oakley, 2011: 189) after she became disillusioned with economics. Wootton remembered that at the time she graduated in 1919, almost 'the whole of the work that occupied the academics of that period was of a deductive, not an inductive character' (Wootton, 1967: 210-211); a degree in economics had left her 'wholly

unfamiliar with the simplest techniques of social investigation', 'hopelessly ill-equipped to undertake any kind of empirical social investigation' (Wootton, 1967: 210-211). Over the years, her dissatisfaction with economics grew even stronger – in 1938, she published *Lament for Economics*, which 'marked the official end of this career [as an economist] and her coming 'from the cold [of economics] to the relative warmth of sociology' (Oakley, 2011: 165).

However, what Wootton encountered in sociology was hardly much more encouraging. It was not just a lack of training in the empirical methods of social investigation, but also the prevalence of the merely 'critical' as opposed to the actively 'constructive' attitude towards social scientific knowledge. Wootton recalled:

> I have been struck by the adroitness with which the clever student picks holes in other people's theories or proposals and, at the same time, by his apparent lack of any sense of obligation to offer alternatives of his own (Wootton, 1967: 202).

What people expected from the burgeoning social sciences in the early post-war period were some new, fresh and potent alternatives for the solution of humankind's problems. In her book *Testament for Social Science*, Barbara Wootton argued, in a similar vein to Beveridge whom I cited earlier, that the most effective way to achieve this was to employ the scientific method in the study of humanity's most pressing social problems:

> It is no less obvious that this method, which has been so brilliantly successful in the natural sciences is not normally applied to the field of our most disastrous failures […] we ought seriously to ask whether the tool that has worked such wonders in the one job could not be used for the other (Wootton, 1950: 1).

For her, the difference between the natural and the social sciences was a matter of 'differences of degree, rather than of kind; and even these are easily exaggerated' (Wootton, 1950: 3). Moreover, in a direct reference to Harrisson's essay, which I discussed above, Wootton objected to Harrisson's rather implicit suggestion that 'qualitative' approaches were more 'humane' and that there was no place for

quantification in sociology. Indeed it was unthinkable that any analysis of anything could be totally devoid of some kind of quantification:

> Clarification and description, as in biology, normally precede the power to reach quantitative conclusions; but they definitely represent a more primitive level of achievement, if only for the reason that the significance of a qualitative conclusion cannot be assessed unless we have some means of judging how far it may be treated as typical. When Mr. Harrisson pleads the value of interviewing without formal questionnaires or the study of case histories of overhead conversations, as sociological tools *alternative* to quantitative methods, he overlooks the fact that the value of the results obtained by these methods entirely depends upon their quantitative significance. […] The social sciences are not at all the victims of 'quantitative obsession'. Like other branches of science, they cannot advance beyond quite elementary stages of development without the use of quantitative instruments (Wootton, 1950: 46).

It is important that instead of merely evoking the usefulness and power of statistical analysis, Wootton pointed to the underpinning value of the statistical and probabilistic *way of thinking* about social and natural phenomena. After Wootton lost in the competition for the LSE chair and ended up teaching social studies in Bedford, the chances of her influencing the future of sociology diminished. At the same time, her way of thinking was becoming increasingly unpopular so that even those social scientists who had a more central position within British sociology than she did, struggled to influence the course of British sociology in a direction that that was more appreciative of empirical and quantitative work.

David Glass

One person, whose views about the scientific base of social science correlated with those of Wootton, but who, unlike her, made it into the LSE as a Professor of Sociology in 1949 (and took the Martin White professorship in 1961), was David Glass (1911-1978). Glass' intellectual interests and academic background lay primarily in demography – he worked in the Population Investigation Committee from 1936 till his death in 1978; in 1944 he was appointed member of the Statistics Committee of the Royal Commission on Population and in 1945 he was appointed a

Reader in Demography at the LSE (Wise, 1983: 205-9). He was, however, a broadly minded intellectual committed to social science and this is what led him, eventually, to an academic career in sociology. But Glass' sociological profile is far from being straightforward. Throughout his career he remained on the threshold of sociology, not quite in, not quite out, always between sociology and what was informally known as the British empirical tradition of social enquiry. And so examining Glass' career is important not only because of his legacy and contributions, but because the contradictions that we observe in his career represent in miniature the contradictions that existed in early post-war sociology as a whole.

Glass himself wrote very little, if anything, on the state of sociology as an academic subject. Although there is no written records of his views of social science and understanding of sociology, there is one piece of evidence which is revealing – the lecture he gave at his inauguration as a sociology professor in 1949. The title of the lecture itself speaks volumes about Glass' priorities –'The Application of Social Research' – and his corpus of work illustrated clearly that it was applied research that Glass found most fruitful and rewarding. It is unclear how his lecture was received at the time but, with the benefit of hindsight and the knowledge of the direction in which sociology was heading, we can imagine that his recommendations for closer working relationship between government and social research were not welcomed. It must have been awkward for Glass pursuing this credo *as a sociologist* – as Westergaard remembered in 1979:

> there were to be critics who read into this distinctive conjunction of problem and policy orientations with meticulous empirical investigation a piecemeal pragmatism unattached to theory, a boost for number crunching sociography more than for some autonomous sociology capable of capturing the essence of social relations (Westergaard, 1979: 173).

Others remembered that at a time when the interest in sociological theory was gaining a strong momentum as the priority approach for British sociology (see below), Glass was 'more interested in the discovery of new facts than in the construction of theories' (Grebenik, 1979: 12); in his work, 'he did not set out to test any grand theory but was concerned rather to measure, record and explain' (Borrie, 1983: 546); his priority was the 'thorough examination of factual evidence, wherever

possible quantitative evidence, rather than theoretical constructs or preconceptions' since this was the only legitimate basis of social and political judgment (Grebenik in Borrie, 1983: 551). This shows clearly in Glass' message for the sociology students, which, again, was similarly at odds with the messages coming from the majority of professional sociologists. Instead of producing well-rounded intellectuals, Glass believed that the role of universities in providing a sociology degree was to enable 'students to develop an imagination which grasps the relevance of social research' and encourage them to 'acquire the knowledge and experience which qualifies them to undertake it' (Glass, 1950: 29). In other words, students should know where best to apply their sociological knowledge and have the skills to do it. Some sociology students enthusiastically embraced his position: most notably, Glass was 'the main influence' on Olive Banks, who 'was immediately attracted by his kind of sociology, particularly its combination of empirical rigour and social policy' (Banks in Banks, Deem and Earnshaw, 1980: 70).

The majority of younger sociologists, however, did not identify with Glass' 'kind of sociology'; in fact, the empirical studies of social science 'had been neglected by economists, and unfortunately also by many sociologists' (Grebenik, 1991: 19). With interests such as these, it was:

> hardly accidental that he should have been attracted to population studies, the branch in the social sciences in which the empirical tradition is strongest and measurement less difficult than in other fields' (Grebenik, 1979: 12).

The number of obituaries written by colleagues and friends after Glass passed away that struggle to pin point Glass' place in social science is representative of the uneasiness with which British sociology approached empirical social research, especially quantitative survey research, and vice versa. It would be misleading, said Westergaard 'to distinguish between Glass the sociologist and Glass the demographer. The study of population for him was part of the social scientific enterprise inseparable from the rest' (Westergaard, 1979: 176). But it had to be acknowledged that as British sociology developed and expanded in the post-war period, it became, at least as far as its mainstream circles were concerned, an increasingly unwelcoming environment for people like Glass:

> […] although he read widely in sociological literature, Glass did not cultivate an interest in that direction [i.e. sociological theory], and he was much less at home in the academic world of sociology in recent years than he had been in the early days of the British and the International Sociological Associations, in which he had taken part. He was extremely critical of some of the products of current trends, whose quality did not come up to his rigorous standards, and it must be admitted that his negative judgments were sometimes disconcertingly swift and uncompromising (Marshall and Laslett, 1979: 2).

What Wootton's and Glass' experience shows is that the climate within British sociology in the post-war period was largely unwelcoming towards people with views like theirs; the way their careers developed in relation to sociology give a clear sign that any Wootton-s and Glass-es were more likely to end up as outsiders, an unwelcome minority.

Alexander Carr-Saunders

Another important example of this emerging trend is Alexander Carr-Saunders (1886-1966). Carr-Saunders held two important posts in social science throughout his career – from 1923-1937 he was the first Chair of Social Science at Liverpool; and from 1937-1957 he was the Director of the LSE. He was also one of the social scientists involved in launching the Population Investigation Committee (1936) and played a major role in establishing the study of demography at the LSE (Blacker and Glass, 1966: 369). He wrote little about sociology *per se*, but it is possible to get a glimpse of his thoughts on the subject from a couple of reviews he wrote of Morris Ginsberg's work in the 1930s when Ginsberg was the only sociology professor.

In 1935, Carr-Saunders pointed out that it was difficult to 'write shortly and clearly about sociology' and that when one is dealing with sociology, 'these difficulties are at their maximum, since whatever definition of the subject is adopted, it is of enormous scope and of infinite complication' (Carr-Saunders, 1935: 499). An even greater problem was that many sociologists 'get no further than attempting to prove that their own conception of sociology is theoretically valid and potentially valuable' (Carr-Saunders, 1935: 499). Like Glass, Carr-Saunders struggled to

understand the direction in which sociology was heading; he saw in the work of Ginsberg little more than repeated attempts to define sociology. It would appear, therefore, that quantitatively oriented social scientists were intellectually uneasy with sociology not simply because quantification was not perceived as essential in sociology, but because they had difficulties grasping the philosophical and theoretical aspiration of mainstream sociologists. The big question in the 1930s, for social scientists like Carr-Saunders was whether sociology, as portrayed by the academic sociologists, actually *existed*:

> […] it [Ginsberg's Sociology] is a sketch of a subject which can be called sociology. […] Is sociology wholly reflection upon the results of other social sciences, or does it engage in field work of its own as well? It is usually claimed that sociologists do both. If so, where is the field? It would seem that sociologists claim to wander freely of the whole territory of social experience. […] It is one thing to welcome a book like this, and another to admit that it is an argument for, and far less a proof of, the existence of sociology (Carr-Saunders, 1935: 499-500).

Carr-Saunders' views of social science were at odds not only with sociology's intellectual development but also with how the subject, in the way in which Ginsberg conceived it, could possibly thrive in academia. The question of 'how far is it possible and desirable to group together the study of the special topics and of social life as a whole?' (Carr-Saunders, 1933: 93) found no clear answer in Ginsberg's, or anybody else's sociological writings and had the effect of discouraging quantitatively minded social scientists from engaging more seriously with sociology. But this is not to say that they completely rejected its value. In his Presidential Address to the Royal Economic Society in 1958, Carr-Saunders argued that sociology, as the general social science Ginsberg was advocating, did have a role to play in the study of society because 'the social world is not readily seen as one world' and sociology could make a contribution in this direction; but he also hastened to 'calm' the 'fears' of the economists by adding that this was not 'that kind of sociology, which aims at transcendence over the special social sciences' (Carr-Saunders, 1958: 448). But even in 1958 it was not immediately clear how quantitative social science would fit into sociology. 'He was greatly curious, but also dubious, about sociology', commented David Caradog Jones and T. H. Marshall (1967: 278) and that perhaps sums up well

the position that many quantitative social sciences held with respect to academic sociology.

What do these examples tell us about British sociology 'at the crossroads'? There were other social scientists, in addition to Wootton, Glass and Carr-Saunders who also advocated the use of quantitative methods in sociology. Support for a more quantitative and empirical approach to sociology came from Tom Bottomore who insisted that 'such advances as has been made in sociology have been due to the argued rise of the ordinary methods of science' (Bottomore, 1962: 45); and from Quentin Gibson who insisted that the approach used in the natural scientific enquiry based on, among other things, reliance on empirical evidence, generalisation and aspiration for objectivity, not only applies to social science but is its only option, if social science were to have real impact on the lives of people in society (Gibson, 1960: 3). Other contemporaries suggested that the first step to accommodate these principles into sociology was a change of attitude and disposition – students had to learn and be convinced that all sociological statements must 'ultimately be related to empirical research'; 'they must come to think in a factual-analytical framework' (Liggett and Wakeford, 1964: 205).

So Wootton, Glass and Carr-Saunders were not entirely solitary figures; but for all their importance in the history of sociology, they have rarely been 'at its centre'; if they have exerted some influence, this has been done from a distance. And the type of understanding, manifested in the writing of those most avidly concerned with the future of sociology as a subject in the early post-war period, like Tom Harrisson and T. H. Marshall, has made it clear that one of the reasons why the Wootton-s and the Glass-es have remained on the sidelines is their insistence on the importance of quantification in sociology.

In 1962, T. H. Marshall declared that his anxiety 'lest sociology be swamped in a flood of [quantitative] studies' 'has diminished but has not entirely disappeared' (Marshall, 1962: 32). He himself was now eager to find a more effective way of combining rigorous quantitative analysis with a solid underpinning of theoretical understanding, citing, most notably, some of the studies that were conducted on the subjects of social stratification and social mobility as examples where this had been successfully achieved. But more often than not, Marshall's injunction that

'sociological interpretation must go *beyond* statistical correlation' (Marshall, 1962: 37) was interpreted to mean that sociological interpretation must go *without* statistical correlation. Too often, the early debates on sociology's path targeted quantitative methods as an extreme position, even though the advance of the quantitative tradition in British sociology was negligible; communication with the few social statisticians which existed at the time was not attempted and instead the focus shifted to the introduction of new methods for reasons that, as Harrisson's case showed, had little to do with enhancing methodology and improving co-operation between theory and method. We could perhaps view Harrisson's arguments as a strategy for making the case for 'qualitative methods' in British sociology. But not only was this done without any proper understanding of quantitative methods, it also relied for its success on a heavily distorted image of statistically based social research. Moreover, it was distorted for reasons that had no direct grounding in the British sociological experience but drew heavily on impressions of American sociology. It was this kind of uncommunicative, distorting, impression-based debates that were turning the development of statistical sociology in Britain into an ever more remote possibility. As Oakley points out, at the time Wootton's *Testament*: 'fell mostly on resistant ears' and it was soon to be forgotten since the empiricism 'of the kind the book recommended was well on the way to becoming a dirty word' (Oakley, 2011: 198).

Thus, as the following chapters will show, in our journey through the history of British sociology we will encounter a majority of Marshall-s, who did not necessarily oppose quantification in social science but did not regard it as an essential element of sociology because the most important contribution of the social sciences lay *elsewhere*; an influential minority of Harrisson-s who were vigorously opposed to any sort of a quantification; and a handful of Wootton-s, Glass-es, and Carr-Saunders-es on the borderline of sociology, who maintained that sociology, or any other social science for that matter, could not succeed in achieving its higher goals *without* quantification.

The Understanding of Methods and Theory in British Sociology in the Early Post-war Period

An examination of the careers and ways of thinking of some of the major figures in British sociology in the early post-war period is an appropriate way of introducing the intellectual climate in which sociology was developing and the methodological choices that sociology was making. However, it is certainly not enough to explain why people like Harrisson spoke the language of mainstream sociology while the views of people like Wootton and Glass were rejected or only reluctantly accepted. What was going on in sociology more generally at the time? What kind of understanding of methodology influenced the path that British sociology would choose when it approached Marshall's crossroad?

A good way to find out is to examine how sociologists in this early period understood the nature and role of methods and how this influenced their way of thinking about quantitative methods. Contemporary observers have argued that, overall in the 1950s, there was a 'strong philosophical bias which assumes the student ought to be interested in social philosophy and capable of considering philosophical problems raised in the study of methodology' (MacRae, 1953: 80). Further insight comes from the 1956 BSA conference on 'The Present State and Development of Professional Sociology'. The nature of sociology and the role of methodology was one of the most widely debated topics there. Some sociologists at the conference expressed a belief that sociology aught to provide students with an opportunity to learn as great a variety of skills as possible because:

> the type of person who was required for research and planning was one with a wide range of reading, verbal and social skills and a knowledge of research methods based upon a study of past classics of social investigation and upon a direct experience of empirical research (Tropp, 1957: 291)[20].

The reality, however, was quite different: Mark Abrams expressed 'bluntly' his disappointment that sociology graduates 'were inadequately trained for any kind of independent empirical research' (Tropp, 2011: 3). Only a minority of sociologists

[20] Asher Tropp wrote an account of the 1956 conference. Two versions of this account have been published – a shorter version in 1957 and a more detailed version in 2011.

'emphasised that sociology could not ignore the vocational aspect' and that university departments 'must necessarily consider the fields in which their graduates would be employed' and design methodology teaching accordingly (Tropp, 1957: 292). Other speakers, again in the minority, insisted that sociology 'was essentially an empirical study' and that instruction in 'sociological methods of investigation could not then be left to the postgraduate stage' (Tropp, 1957: 292).

The majority, however, believed that the vocational element and with it, practical methods classes and instruction in how to do different methods, was not a priority for sociological education. It was argued that society 'should not only be considered factually and empirically' and that 'social philosophy was an indispensable part of the training of sociology graduates (Tropp, 1957: 292). The overall agreement was that the sociology degree 'was broad and humane and not narrowly vocational' and that 'the details of research techniques should be left to the graduate stage (Tropp, 1957: 292). The broad consensus, as summarised by Tropp was that:

> The university teachers were agreed that the correct syllabus for undergraduates and graduates had to be determined in the light of the universities' duty to provide a general education and to train people to contribute to the advancement of sociology. Undergraduate training should continue to be broad and humane, leaving it to the graduate stage to combine a continuation to this broad education with the training in advanced sociological theory and research methods (Tropp, 1957: 293).

Accompanying these well-balanced remarks were comments showing that the conclusions reached by the sociology teachers were based not only on rational deliberation but also on the idea that anything that does not contribute to an abstract and all-encompassing theory or philosophy of society is trivial and not a desirable path for sociology. So; Tropp reported that underlying some of the comments and coming up again and again in the discussion was:

> a deep concern […] with what some members considered to be a trivialisation of sociology and a retreat from the consideration of significant social problems into the waste-land of methodological rigour and ethically neutral theory' (Tropp, 2011: 3).

Therefore, in the 1950s, a majority of British sociologists not only regarded methodology as an unnecessary addition to the undergraduate curriculum and something that should be reserved for the postgraduate stage; they also showed a more general lack of appreciation of 'methodological rigour'. It is such broad underlying views that would prove to be a powerful factor in the overall lack of appreciation of quantitative skills that would persist in academic sociology despite any changes in the specifics of the sociology curriculum[21].

It has been argued, albeit rather impressionistically, that the 1940s/1950s were a period in which British sociology was characterised by empirical, if not empiricist, work (for an overview, see Platt, 1981). Although, as I mentioned above, the early post-war period did see a shift away from the social philosophy of Hobhouse and Ginsberg towards more 'social-problem-based', empirical work, it was not long before this kind of work was coming under attack for making 'no use of sociological theory, either in the problems selection or in their treatment of the problem' (Little, 1963: 91).

There is, indeed, substantial evidence to suggest that throughout the post-war period, a primary concern for British sociologists was the development of a sociological theory, not empirical work. A survey of sociological research conducted in 1956 showed that 'few research projects have methodology as their primary object' and that

> analysis in terms of articles published in the two sociological journals [Sociological Review and BJS] or in terms of books reviewed in the same two journals might suggest that the extension of social philosophy was the primary activity of sociologists (Madge, 1957: 86-87).

Despite the fact that Hobhouse and Ginsberg had been working on a type of sociological theory, their work was now beginning to be dismissed as irrelevant, leaving the impression that theory was an area of sociological enquiry that has been previously *neglected*: 'it is perhaps an unfortunate thing that there has not developed, even within the universities, a body of sociological theory' (Banks, 1954a: 50). Some

[21] This particular understanding of methodology was linked to sociologists' understanding of sociology as an academic subject – more details of how this manifested itself in the academic curriculum will follow in the chapter on teaching below.

of the first post-war British books on sociology were devoted to sociological theory – Donald MacRae's *Ideology and Society* (1961) and John Rex's *Key Problems of Sociological Theory* (1961). Although they were seen as an important step forward, they were also seen as a step that underlined the absence of work on this level. Given these circumstances, theory, it was argued, deserved sociologists' utmost attention: 'the reiteration that the building up of an adequate body of theory is the vitally necessary step in the development of the subject' (Banks, 1954a: 50); there was 'an urgent demand for the creation of a more adequate theoretical approach to our work.' (Simey, 1957: 128). It was a common view that 'theoretically oriented studies are generally greatly superior to descriptive studies in their economy of effort' (Cotgrove, 1978 [1967]: 29) and that the job of a sociologist was to 'build a body of theory to explain the behaviour of social systems' (Cotgrove, 1978 [1967]: 38).

A survey of sociologists conducted by Carter in 1967 revealed that similar beliefs were shared right across the sociological community. In 1967, about 28% of professional sociologists (i.e. members of the BSA and not members of the BSA but engaged in teaching and research in sociology) expressed interest in 'basic theory', compared to only about 12% who expressed interest in 'methods and methodology' (unfortunately, it is not clear if there was an overlap between these fractions, so it is not possible to assess how many sociologists, if any, expressed interest in both) (Carter, 1968: 15). In addition, theory topped the list of sociological fields that were perceived as having been neglected – 13.7% of respondents reported theory as the neglected field, compared to 3.6% who reported that 'methodology' has been neglected and 4.8% who reported that 'methods and techniques' have been neglected. Theory was also the field in which sociologists believed growth was most necessary – 20% of respondents reported this; while the proportion of those who believed that growth is necessary in 'methodology' and 'methods and techniques' was negligent, only 1.2% and 4%, respectively. Less than 1% believed that mathematical sociology has been neglected; only 3.4% thought growth in this field was necessary. 'Research must be 'theoretically problem oriented' as opposed to 'social problem oriented', was one of the widely spread beliefs according to Carter's survey (Carter, 1968: 35). So; if there was a shift in attitudes in the early post-war period, this was not a shift *away* from theory but was a shift towards a different *kind* of theory – 'the need for a

better theory of this special kind should be regarded as establishing the case for better experiment, rather than deeper contemplation' (Simey, 1957: 128). It was a move away from the grand, abstract social philosophy developed by Hobhouse and Ginsberg and towards a more moderate, smaller-scale, theoretical work.

Although many early discussions in British sociology contained a similar element of the importance of bringing theory and method together it remained unclear how 'better' experiments could be done or taught given the negligible amount of teaching of methods that persisted in sociology undergraduate curricula (see Chapter Twelve below) and the little interest in methods and methodology expressed by sociologists at the time. Nonetheless, it was not uncommon in the early 60s to argue that:

> The future of sociology may depend more than anything else, on the question whether statistical survey techniques can be married to the complex but often inexplicit techniques of personal observation and description (Townsend quoted in Marshall, 1963: 26).

But, often, the desire to bring theory and methods closer together was accompanied by an understanding that the development of sociological theory should come first and attention to methods, especially quantitative methods, later: although the marriage of techniques was something that was 'certainly desired', it itself 'cannot give birth to a science'; 'something more was needed' and that was the development of sociological theory (Marshall, 1963: 26). The 'preoccupation with theory' could be justified and was 'reinforced by the increasingly urgent need for fruitfully oriented research' (Fletcher, 1957: n.p).

How can we explain the increasing attention given to the need to develop more and better sociological theory? An inter-play between two important factors was at work. First, as I briefly mentioned above, a common and widely held view was that British sociology had not produced any major theoretical breakthroughs in the first half of the twentieth century: it was argued that 'British sociologists on the whole tend to be […] *eclectic rather than particularist in the building up of theoretical schemes'* (Banks, 1954a: 50), with the theoretical work of the kind that Hobhouse and Ginsberg had been working on being seen as unsuccessful. Now that sociology was slowly becoming more popular and beginning to expand in the universities, the

chances for the development of theory were perceived as being greater, and as the above discussion showed, there was an overall agreement that the focus should be on theory.

Intertwined in these views was also a perception that British sociology had already developed empirically. Banks commented that 'British sociologists on the whole tend to be empirical rather than analytic', (Banks, 1954a: 50) while Madge argued that in Britain there was 'a substantial corps of empirical sociologists (Madge, 1957: 87). Where did this impression come from, given that the work done under the name of sociology since the beginning of the twentieth century was primarily of a social philosophical type?

A clue comes from the examples that Banks and Madge list to support their views. The advances that Banks and Madge argued were being made in empirical social enquiry were not made as part of 'academic sociology' and were not made by professional sociologists. For instance, both Banks and Madge mention the LSE Research Techniques Unit as an example of 'a prominent exception to this disinterest in method' (Madge, 1957: 87). This research unit, however, was established in 1949 by Maurice Kendall, a professional statistician. The unit comprised mainly of professional statisticians (Claus Moser, Alan Stuart, James Durbin) and psychologists (William Belson) (Banks, 1959); other bodies, such as the Social Research Unit at the LSE and the Population Investigation Committee contributed to British empirical social science, but were not strictly sociological institutes; the PIC was a demographic organisation (Banks, 1954b). The rest of the bodies mentioned by Banks (The Medical Research Council, Tavistock Institute of Human Relations, National Institute or Economic and Social research) and Madge (Government Social Survey) were even further removed from academic sociology. Some sociologists even referred to the advances achieved within the British statistical tradition when they were trying to promote sociology: in a guide for intending students in sociology and social anthropology, one of the first such guides in the country, Banks and Tropp argued that sociology and social anthropology had their origins in the work of the political arithmeticians William Petty and John Graunt and in the work of the London (Royal) Statistical Society; this was really when 'first hand investigation of

social phenomena took over from philosophical enquiries' (Tropp and Banks, 1960: 1).

But the historical reality is that direct links between academic sociology and the nineteenth-century British empirical tradition of social enquiry have always been weak to non-existent. Thus although the British empirical tradition of *social science* cannot be ignored when we consider the study of society as whole, it is important to stress that *in the history of sociology in Britain* this tradition remained distinct from British *academic* sociology. Even Madge acknowledged that if one was searching for methodological findings, one would struggle to find them in sociology journals, which were 'rather bare' in this respect (Madge, 1957: 88); but one could easily find numerous contributions of this kind in statistical or even philosophical journals.

It becomes clear then that the impression that some post-war sociologists shared about British sociology's apparent preoccupation with empirical work comes merely from the fact that they were inclined to accept the contributions made in other areas of social science as part of a 'common sociological heritage'. What is more important, however, is that the divide between this empirical tradition and academic sociology remained unquestioned; *instead, the fact that empirical work was being done elsewhere was understood as a legitimate reason why sociology should direct its own efforts towards more theoretical, radical or critical enterprises.*

It could be argued then that, since Madge and many others were prepared to think, at least to an extent, of the contributions of empirical social enquiry as contributions to sociology, the divide between sociology and empirical social science is irrelevant; a historiographical fiction that is important in the world of pernickety historians but not the sociological community. However, the divide between the type of sociology that developed academically in Britain and the empirical social science that was being done elsewhere matters greatly when it comes to the teaching of sociology to undergraduate and postgraduate students, who would then go on to form another generation of professional sociologists. If sociology students were taught by sociologists who did not perceive empirical enquiry as essential and themselves did not possess adequate skills for conducting such an enquiry, especially quantitative empirical enquiry, then the students themselves would be likely to acquire similar attitudes and values and lack the necessary skills to conduct such an enquiry, despite

that fact that they were continuously taught research methods. The chapter on teaching below explains exactly how this came to be the case for the majority of sociology students in the post-war period.

British Sociology's Views on Empirical Social Science in the Early Post-war Period

Mixed with the view that, since empirical work was being done elsewhere, it was desirable that sociologists direct their efforts to the development of sociological theory, was a particular perception of empirical social science. British sociologists in the early post-war period were, on the whole, depreciative and sceptical about empirical enquiries, especially quantitative empirical enquiries. For instance, in the above example in which Madge acknowledged the contributions to empirical social enquiry made in the LSE Research Unit, he did not hesitate to criticise the value of such work to sociology:

> While much of value has emerged, the research techniques so far explored appear somewhat narrowly based, concentrating as they do on the statistical sampling and administrative problems of mass interviewing and displaying rather little curiosity about the art and science of asking questions or of the other methods of inquiry open to sociologists (Madge, 1957: 87).

However, what exactly is 'the art and science of asking questions'? Were sociologists themselves able to offer their help to social statisticians in this respect? Madge does not make this clear; what *is* clear is that however imperfect empirical and statistical work was, sociologists like Madge tended to emphasise its imperfection rather than its potential.

The 1957 BSA conference entitled 'Sociology in Retrospect and Prospect' was a fitting opportunity to discuss the role of empirical social enquiry in sociology and provides further insight. The views discussed at the conference show a tendency to question the success and quality of the works of the most prominent empirical social

scientists of the nineteenth and twentieth century, such as William Farr, Charles Booth, Seebohm Rowntree and Arthur Bowley. It was understood that:

> The dawn of the new empirical sociology turned out to be a false dawn, partly because the system builders went on to building systems, and to a large extent crowded the empiricists off the stage (Simey, 1957: 125).

Note that Simey called the work of Farr and Booth etc. not empirical but 'empiricist' which in this intellectual context is a derogatory term. Although he admitted that the application of random sampling in social surveys has undoubtedly been a step forward, Simey questioned the overall value of the empirical social studies: 'when attempts have been made to develop scientific methods still further […] the results have been more questionable (Simey, 1957: 125).

Another good example that sums up this mood of suspicion towards past and contemporary empirical work in the 1950s, comes from W. J. H. Sprott, who criticised the empirical tradition of social enquiry, pointing to Hobhouse and Ginsberg as the only ones who managed to withstand its malign influence:

> Let us not rely in superficial appearances; let us find out the facts. This is the mood of British sociology: fact finding. […] Two figures, and I think only two, stand for something different. They are L. T. Hobhouse and Morris Ginsberg (Sprott, 1957: 608).

However, since Hobhouse's and Ginsberg's work failed to attract followers and, it was argued, failed to bring any major theoretical advances to British sociology:

> Theory and encyclopaedism are for the moment unfashionable […] It is not the Hobhouse tradition that we follow, it is rather the fact finding tradition of Booth. Research is in the hands of the applied sociologists, not in those of the theorists (Sprott, 1957: 609).

The 'fact finding tradition of Booth', Sprott argued, had been, for too long now, *inhibiting* sociology from realising its full potential especially with regard to sociological theory:

> I think myself that a study of sociological theory is important and it is neglected in this country. For its development we require […] sociologists who aim at making their research serve their theories, rather than hanging around making themselves useful. […] this is a minority opinion. […] My own hope is that among the increasing number of students who study the subject some may come forward, endowed with scientific imagination, who will refrain from knocking at doors and presenting questionnaires until they have formulated some theoretically significant hypothesis for verification (Sprott, 1957: 622).

The study of sociological theory may have been perceived as important by a 'minority', as Sprott says, but a minority of social scientists, not a minority of sociologists – in the 1950s there were still a lot more people involved in empirical research at research units and social science department than there were academic sociologists. The majority of academic sociologists in 1950s and early 1960s saw empirical work as 'not-sociology' but as mere fact-finding into 'the condition of Britain' question' which aimed 'to discover facts for governments and to influence policy and legislation' (Cotgrove, 1978 [1967]: 29). It was argued that the application of statistics 'has become a fashion, if not a cult'; that 'more excessive reliance is placed on it' and that 'it represents perhaps the most misleading technique ever relied on by social scientists' as a result of which many social scientists 'have gone far up blind alleys in this way and resolutely remained there' because they have retained their belief that 'there is something innately scientific about work of this kind' (Simey, 1957: 126). In addition to being a product of almost entirely 'pragmatic' concerns, such as the solution of social problems, empirical social scientists, especially those conducting survey research, were misguided in their belief that 'the task of sociology was to establish statistical regularities' and were thereby failing to offer an understanding of 'the meaning which the action has for the actor' (Cotgrove, 1978 [1967]: 34). In addition, it was argued that any attempts to understand society statistically, following procedures that were used in the natural sciences, could only make a very limited contribution to the advance of sociological knowledge – it was one thing, Cotgrove argued, to deal with millions of molecules 'which are relatively simple' and a completely different thing to analyse 'a system made up of a few hundred very complex men and women' (Cotgrove, 1978 [1967]: 34). While the study of the former, due to their simplicity, allowed natural scientists

to reach results of a much greater certainty, the study of the latter, due to their complexity, yielded results marked by a high level of uncertainty.

Those with a much better-grounded knowledge in statistics would most likely disagree with Cotgrove, pointing out that the level of certainty of the results of a statistical study depends on the way the study has been conducted, not the objects of the study. But rarely, if ever, did sociologists refer to the work of statisticians or consult them before making their claims of inadequacy. It is not exactly clear what sociologists like Cotgrove based their statements on; but it is clear that in the early post-war period, little effort was being made to foster better communication between sociologists and statisticians that would have helped sociologists to improve the quality of their arguments about statistical methods. It may be true, as the statistician Claus Moser argued in 1954, that as far as good sociological practice is concerned, the conflict between theory and method is 'entirely illusory' and that 'fundamentally, they should be complimentary to each other'; but, as he pointed out, in real life, there was 'a lack of communication, if not actually a conflict, between them' (Moser, 1954: 90).

The 1967 survey by Carter quoted above that showed a common and widely spread desire among British sociologists to encourage the development of sociological theory, also showed that scepticism and hostility towards empirical and quantitative work was common. Carter reported that 'an interest in Methods and methodology has not been and it is not, a major feature' (Carter, 1968: 16); although there were 'signs that increasing numbers of sociologists are attaching considerable importance to both of these aspects [methods and methodology]', there was a clear but 'perhaps less powerful, current of opinion against what is perceived as the danger of 'methods men' taking over the subject and reducing it to 'computerised trivia' (Carter, 1968: 16). More extreme views were also expressed, going as far as to argue that empirical and quantitative research was not sociology at all and that it was not done by sociologists but by some researchers called 'the hybrid type' (Marshall, 1953: 208). In sharing his impressions of the first BSA conference in 1953, Marshall, agreeing with Ginsberg, argued that 'much of this work, useful and necessary though it is, ought not really to be called 'social science' at all, nor are those who do it in any real sense 'social scientists' (Marshall, 1953: 208).

For contemporary statisticians, it was obvious that statements like these were misguided, based on insufficient knowledge and showed a clear lack of will to engage with statistics. One of the most prominent social statisticians of the time, Claus Moser, addressed sociologists' claims, showing that their criticism of statistical practices was correct only as far as *low quality* social statistical research was concerned:

> The purpose of many surveys is simply to provide someone with information. […] In this early, fact-finding, stage of the social sciences there is virtually no limit to the range of topics covered by surveys. It must not be thought, however, that the purpose of surveys in social research or elsewhere, is always so straightforward. Many enquiries are aimed to explain rather than to describe. Their function may be theoretical – to test some hypothesis suggested by sociological theory – or severely practical – to assess the influence of various factors […] It is the ill-considered launching of surveys, leading to the waste of much time and money and the accumulation (often) of unwanted data, that has given rise to the scepticism with which some sociologists regard "door-knocking" research (Moser, 1967 [1958]: 3).

Sociologists who criticised statistical social enquiries were equating all statistical research with 'ill-considered launching of surveys', without acknowledging the advantages of the 'well-considered' launching of surveys. Such attitudes did nothing to help bridge the gap between theory and method but served only to make any potential co-operation between sociologists and statisticians more difficult.

There is some evidence to suggest that those working in the field of social statistics were, generally, more open to improving communication with the sociologists than vice versa. Even before Moser's explanatory remarks, another prominent statistician and holder of the new second chair of statistics at the LSE (1949-1961), Maurice Kendall, made some conciliatory remarks which were partly aimed at the sociologists. Kendall acknowledged that the statistical approach had not been regarded as particularly important to sociology; but he also expressed his views regarding the possibility of a closer co-operation between statisticians and sociologists:

> The statistician is not, and makes no claim to be, a sociologist, a politician or a moral philosopher; but he may, perhaps, be allowed to suggest that the statistical approach, in the sense I am using the term in this lecture, should not be overlooked in the subjects with which they are concerned (Kendall, 1950: 136).

In a similar vein, as part of a discussion at the Royal Commission on Population, David Glass, as a statistician who was also working in sociology, recommended that:

> […] There needs to be a much closer link between the statistician and the sociologist in demographic investigation that has been general in the past. It is not just a question of handing to the sociologist the task of studying fertility in social context. It is equally important that the sociologist should help to frame concepts and definitions which may result in more meaningful indices […] (Glass, 1951: 44).

There might not have been many social statisticians like Kendall and Glass who openly advocated for a better communication with sociology and made efforts to establish such communication. But there were fewer still, if any, mainstream sociologists prepared to be as conciliatory towards statistics.

Whether British sociologists' beliefs about statistics, surveys and empirical work more generally were well-founded or in any way justified, is a topic for a further discussion. For the purpose of the present discussion, it suffices to say that what British sociologists *chose* to believe about empirical social science in the early post-war period was bound to have important implications for the beliefs and attitudes of the generations of young sociologists to follow.

Economics and Statistics in Britain in the Early Post-war Period

In the early post-war period, British sociology was, at least to a degree, exceptional in its lack of willingness to develop an empirical programme and teach quantitative skills. Economics, for instance, was incorporating statistical techniques into research and teaching with much greater enthusiasm and vigour. Writing in 1950, G. D. H. Cole, a political theorist and economist, noted that the teaching of

economics had been changing rapidly in recent years, pointing out 'the increasing use of statistical methods wherever they can be invoked' (Cole, 1950: 34). Cole's observations were corroborated by the findings of Guillebaud (1953) who, having analysed the contents of the economics degrees across higher education institutions in the 1940s (Cambridge, Oxford, LSE, Manchester, Birmingham, Glasgow, Swansea) showed that statistics was a component of every economics degree. In most of these institutions, elementary statistics was a compulsory subject within the economics degree; the LSE set an important example with a compulsory statistics paper in the Intermediate examination (after second year) and a compulsory statistics paper in the final examination for BSc Economics degrees with special subject in the field of economics (as opposed to, for instance, history, sociology and social anthropology, where there was no statistics paper). All degrees in economics offered the students, if they wished, the opportunity to develop higher-level skills in statistics, going beyond introductory courses. In summary, Guillebaud reported that:

> There has been a very marked growth in recent years in the appreciation of the importance of knowledge of statistics and of statistical method and technique […] The number of teachers of statistics has increased rapidly during the last few years, and they constitute today an important element in the economics departments of universities (Guillebaud, 1953: 37).

In contrast, it was observed that:

> […] there has been very little statistical work or teaching of statistics related to either politics or sociology; yet statistics can be fully as useful in these fields, and statistical methods are needed in them in order to combat tendencies towards untested generalisation' (Cole, 1950: 34).

Cole went on to admit that the teaching of sociology was still 'a very difficult question' and made reference to a tendency that was starting to develop at the LSE (and other institutions offering sociology), and which he called a 'bad habit', of 'calling by the name of sociology all the odds and ends of Social Studies that we can't conveniently bring under any main head'; anticipating the later expansion of sociology, Cole urged that the development of sociology 'should be slow and tentative since there is hardly anyone to teach it' (Cole, 1950: 36).

But not everyone was prepared to distinguish between economics and sociology on the basis of the provision of methodological training. F. S. Stone, for instance, argued that the training in methods in the undergraduate curriculum of economics, sociology and politics alike is 'rudimentary' or 'non-existent'; even a first-class degree was not a guarantee that the graduate will be able to undertake empirical investigation or even the most basic quantitative analysis (Stone, 1947: 280). An analysis of the LSE curriculum (see Chapter Twelve) tends to support Cole's and Guillebaud's conclusions more than those of Stone, as least as far as the LSE was concerned. Unfortunately, a full survey of the teaching of economics is not possible here; and we should, of course, note, that Stone's and Cole's and Guillebaud's accounts are a product of different observations and different standards as to what exactly constitutes 'rudimentary skills'. But for the purpose of the current analysis, it suffices to say, that even if economics graduates were not coming out better equipped than sociologists, the systematic presence of quantitative analysis in Economics, at least in leading institutions such as the LSE, nourished an attitude which was much more appreciative of quantitative methods than was the case in sociology. The presence of quantitative methods was much less controversial in economics than it was in sociology.

Intellectual Debates in Sociology and Statistics in Britain, 1930-1970: Summary

This chapter showed that in the early post-war period there was a widespread fear, however unlikely this was, of British sociology falling into 'an obsession with empiricism'. Empirical research and quantitative methods were portrayed as a fruitless path for the future development of British sociology and a distraction from what ought to be the real priority – the development of a sociological theory. This period was, in fact, dominated by a revival of interest in sociological theory (but a different kind of theory from the theory developed during the interwar period). It was also dominated by the rejection of the British empirical tradition in social research that had existed during the nineteenth and early twentieth century as un-sociological,

in spite of the fact that there was still little clarity as to what 'sociology' actually was. The advantages of the ability to think about and analyse sociological data statistically were rarely discussed; instead, quantitative methods were regularly equated merely with techniques for gathering information; as something that could wait until British sociology has grown theoretically. Thus, those like Barbara Wottoon, David Glass and Alexander Carr-Saunders who were positive about empirical sociology and emphasised the benefits of quantification were marginalised.

The relationship between statistics and sociology was shaped first and foremost by sociologists' *attitudes* towards statistics; there is no evidence in the historical record that discussion about the use of quantitative methods in this period ever reached a stage where the focus was on the practical requirements that would make empirical/quantitative research possible, such as the presence or absence of facilities for conducting such research. This suggests that a lack of students willing to learn statistics, or a lack of staff willing and able to teach it or a lack of computer technology to facilitate its inception played a role only in those places where statistical methods had already been *accepted*, where attitudes towards them was welcoming and positive. But, as the following chapters will continue to show, there were few places where this would have actually been the case even as British sociology was reaching the zenith of its greatest popularity amidst a period of unprecedented expansion.

Chapter Eleven

The Expansion of Sociology in the British Education System between 1940 and 1970 and its Consequences for Sociology's Relationship with Statistics

Introduction

So far I have looked at the early post-war debates about the nature and methodology of sociology and analysed the effect these had on the way empirical and quantitative research and techniques were received within sociology. Now I move on to discussing the rapid, large-scale expansion of sociology within the higher education system between 1940-1970 and how the circumstances of this expansion affected the opportunities for the teaching of quantitative methods and for the incorporation of these methods into sociological research.

The expansion of higher education began slowly after the Second World War and peaked in the mid to late sixties largely, but not exclusively, in the aftermath of the Robbins report of 1963. This expansion is the single most powerful factor in the development of sociology in the twentieth century because it made sociology *practically* possible; the intellectual, human, financial and material resources on which sociology as a field of intellectual enquiry and as an academic discipline drew increased not only in quantity but also changed in kind. By the early 1960s, it was argued, two things were 'certain' about sociology – that it 'has become accepted and it has expanded' (Little, 1963: 64); it had 'become an accepted constituent in the teaching and research of British universities' and was now 'deeply implicated in the public practice of doing good' (MacRae, 1957: 98). 'It would now be very difficult to get rid of us', claimed MacRae: 'in 1957 sociology is a magic word' (MacRae 1957: 98).

Examining such an important historical shift poses certain challenges for the historian, one of which is finding the suitable contexts for a proper understanding of the expansion.

The expansion of sociology took place, firstly, within the context of sociology's historical development prior to 1945. The previous chapters have examined this context in great detail, highlighting, among other things that academic sociology in Britain remained weak and ill-defined. This reveals a paradox at the centre of the general picture of the expansion of sociology: how did a subject which existed, both institutionally and intellectually in relative obscurity; which struggled to make itself relevant; and which, at least academically, had restricted itself to a philosophical study of society, suddenly rise to become one of the most fashionable and desirable of subjects? How did a subject that up to 1945 appeared so unpromising, come to be seen as holding out great promise for both university applicants and university vice-chancellors?

From the context of the history of sociology, it is evident that sociology could not have not created the conditions for its own expansion. To help us understand how the expansion became possible, regardless of sociology's past, we have to examine the expansion of sociology in the context of the general expansion of higher education in Britain after 1945. When looked at from this angle, the answer to the question 'Why did sociology expand?' might appear quite obvious – sociology expanded because the whole higher education system was expanding. But this provides only a partial answer to the question. We also need to ask – why did sociology *in particular*, expand? University subjects did **not** expand at an equal rate and the expansion period, especially in the post-Robbins era, was characterised, generally, by a swing *away* from scientific subjects and *towards* "softer options", including the social sciences (cf. Mandler (2017) although I argue below that a slight modification of this view is necessary). But not only was the boom in the social sciences accompanied by a swing away from the natural sciences; of all social science subjects, it was sociology that benefited most. Explanations that limit themselves to the force of the general expansion of the higher education fail to address this more specific issue, which is vital for understanding what made sociology the subject we know today and what enabled sociologists to take a rather neglectful approach towards methodology and the role of statistical methods without losing its popular position.

Another challenging aspect of any historical examination of the expansion is paying proper attention to the things about sociology that changed as result of the expansion and the things that remained the same. This chapter pays special attention to both the differences and similarities, compared to the past, in sociologists' approach to methodology, particularly quantitative methods. It shows that an attitude persisted in which methods training was not regarded as essential for a subject like sociology whose primary function was seen as the provision of a general education. And while previous chapters showed that in their debates about the future of the subject, sociologists did not consider competence in quantitative methods to be essential and did not put it up as a criterion of the sociology degree; this chapter explains why sociologists *could not* have raised quantitative methods training to an indispensable prerequisite in a sociological education if they wanted to sustain sociology's popularity and avoid discouraging students.

But together with the trends that persisted, there were also things that changed. The position of sociology in academia became more secure and the range of sociological topics studied in universities broadened significantly. A broader range of methods was introduced, mainly qualitative methods but, along with this, much pseudo-philosophical discussion about methodology emerged. We will see that the circumstances of the expansion allowed sociology to expand laterally, in terms of topics that could be studied, but not necessarily in terms of methodological competence to study these topics.

These are challenges which no other history of sociology in Britain has attempted to address carefully and in great detail; most histories provide an account of the expansion that is limited to describing its positive effects (cf. Halsey, 2004). What has not been discussed is how to explain the historical meaning of this expansion, both for sociology and for its methodological character; and how to explain the problematic nature of the expansion and its consequences. Most texts focus on sociology in the background of the expansion, and not on the expansion in the background of the history of British sociology or the development of the education system as a whole. So; parts of this chapter will be devoted to exposing this attitude and showing that the complacency with which sociologists approached the expansion at the time had an indirect effect on the way the role of statistical

methods was perceived within sociology. It will also show that this perception of the expansion is still common today.

What sociology achieved in the post-war period academically had little to do with what it had achieved previously; its expansion was not internally driven, the result of a building-up process taking place within the subject itself. This may sound obvious but the fact is that when sociologists have written about the history of sociology in the post-war period, the question of how it was *possible* for sociology to expand has almost always been obscured by the excitement surrounding the expansion itself. Examining this question, however, holds the key to the understanding of the history of the subject in the twentieth century.

Overview of the Higher Education System Leading up to and during the Expansion

The higher education system in Britain in the first half of the twentieth century was predominantly conservative: universities were regarded as 'elite institutions' whose purpose was not the education of the whole of society but the education of a very small proportion at the top; universities were there to educate 'the leaders of society' (Anderson, 2006: 122). This principle not only describes how people thought things *should* be; it was commonly believed that things could not be any other way – 'there was a widespread assumption that the potential constituency for university was limited by innate ability to perhaps 5% of the population (Mandler, 2015a: 2-3).

This conservatism is also evident in the way in which universities were being financed – the University Grants Committee (UGC) was established in 1919 but it funded only about a third of the expenditure of the universities; the other two-thirds had to be funded privately, and research and teaching in the social sciences, in particular, was funded largely by American philanthropic foundations – Rockefeller, Carnegie, Harkness – with the Rockefeller funds playing a key role in expanding the LSE's premises and library as well as funding a great deal of the research done there. This includes Beveridge's social biology project and the *New Survey of London Life*

and Labour in the 1930s (for a full account of the role of American philanthropies in social science funding in Britain in the interwar period, see Fisher (1980, 1983), Bulmer (1982, 1984) and Renwick (2014)). And although the UGC did not interfere with the way the universities spent the money they received, it exercised control on a different level by deciding which universities deserved to receive grants, with existing 'elite' universities like London, Oxford and Cambridge benefiting the most.

Even before the war, however, it had been apparent that 'the graduate output of the universities needed to be largely increased if the national need for men and women with university training was to be fully met' (University Grants Committee, 1953: 17). This growing concern was exacerbated by the large number of men and women returning to civil life from war service in 1945, wanting to continue their higher education which had been interrupted by the war (the driving force behind the first phase of the expansion in 1946/7-1952/3; cf. University Grants Committee, 1964: 65). Suddenly, less than three years after the end of the war, the universities were seeing a 50% increase on pre-war student numbers: while in 1938/9 there were 16 universities and 5 university colleges catering for ca. 50,000 students, in 1947 the same number of higher education institutions had to cater for ca. 77,000 students (Nuffield College, 1948: 85).

Three government reports recommended specific measures to address this situation – the 1944 Goodenough committee on medical education; the 1946 Barlow report on education in science and technology; and the 1946 Clapham report on social science. As a result of the implementation of the recommendations of these committees, in the period 1946-1951/2, the number of Science students doubled, and the Arts (including Social Science) students increased by 60% (University Grants Committee, 1953: 17).

Although there was a gradual expansion immediately after the war, there was little sign that the university system was changing its nature (Anderson, 2006: 131). That the early post-war expansion of higher education did not at first break with the conservatism that characterised higher education during the first half of the twentieth century is apparent from the tone of contemporary discussions of the state of higher education where the *quality* (not quantity) of students was raising major concerns. It was pointed out that, on the whole, 'the number of really first-class men and women

reading arts subjects seems to be slightly smaller than before the war' (University Grants Committee, 1953: 26) and this was interpreted as a warning sign:

> […] no further substantial increase in student numbers could be expected in the immediate future without reducing university standards […] In most universities and colleges not many suitable candidates present themselves for whom places are not available and in some we have been told that "the margin of cultivation" has already been reached (University Grants Committee, 1953: 26).

One of the most common complaints about the changes in the post-war curriculum was that students were 'unduly preoccupied with examinations and that they fail to use their time at universities to broaden their minds' (University Grants Committee, 1953: 45). The greater demand for higher education was seen not only as a challenge, but also – as the tone of the discussion suggests – as a problem, since, it was argued, if fifty years ago it could have been assumed that the general education of the university students 'could be left to take care of itself' since they 'would have come from a home in which he [sic.] would have acquired cultural interests', in 1950s this was no more the case (University Grants Committee, 1958: 39).

Another problem which inhibited the effective provision of high quality general education was the lack of communication which was observed to exist between the sciences and the humanities at all academic levels – 'narrowness of outlook could be found in any specialist, irrespective of subject'; however, 'ignorance of science by arts students is probably more prevalent than ignorance of the humanities by science students' (University Grants Committee, 1958: 37-8). These views received a much wider attention after C. P. Snow's 1959 lecture on the two cultures which built on what the University Grants Committee had observed two years earlier.

Although the 'two cultures' problem when understood as problem of *communication* between science and humanities did not disappear with the expansion of higher education, conservative attitudes *did*. The rapidly growing unsatisfied demand for higher education could only be met if the system was broadened to cover a larger spectrum of the population; simply enlarging the system as it currently was, was never going to work. A second phase of the expansion took place between

1952/3-1961/2 when an increasing proportion of the 17-18 year-olds were staying longer at school and qualifying for admission to universities. As a result of this second expansion phase, termed 'the trend', 'whereas nearly 80% of qualified leavers gained university places in 1956, by 1962 fewer than 60% got them' (University Grants Committee, 1964: 65; Mandler, 2015a: 3). And finally, a third phase took place from 1961/2 onwards when, in addition to 'the trend', the 17-18 year-old age group started to increase following the rise in the birth rate in the later years of the war. All this necessitated a much deeper transformation that we can begin to see during the late 50s and 60s – the democratisation of the British higher education system[22].

The Democratisation of the British Higher Education System

The process of democratisation was manifested in a variety of ways and understanding it is essential for the understanding of what made possible the expansion of sociology, particularly its expansion as a non-empirically and non-quantitatively oriented subject.

I. Changes in the Core Principles of Higher Education

The democratisation of higher education was manifested first and foremost through a change in core principles. In 1939, a shrewd observer commented that the prevalent conception of education was still that 'of a training for the upper classes of society in aristocratic accomplishments' rather than 'the principle of making education available to the mass of the people (Simon, 1943: 32). But by the 1960s, the university system was seen as an 'essentially public' system; part and parcel of the welfare state system (Anderson, 2006: 142). This was manifest in the remarkable

[22] I'm especially grateful to Professor Peter Mandler for an informative and stimulating discussion of the development of higher education in the post-war period, which helped me enhance and extend the arguments I present here. Peter Mandler has written extensively on this topic – for full details on the process of democratisation, please see Mandler (2014, 2015a, 2015b, 2016, 2017).

increase of public spending on higher education – while in 1938 only 38% of universities' income was provided by the state, by 1953 this had grown to 70% and by the mid 60s – to 80%. Therefore, a shift in attitudes towards higher education was taking place even before the famous Robbins report of 1963; but what Robbins did, very importantly, was to legitimise and justify the universal provision of higher education not only for 'for all those who are qualified by ability and attainment to pursue them' but also for those who 'wish to do so' (Robbins, 1963: 8). As one commentator later put it, Robbins' greatest contribution was that it 'validated past and stimulated future expansion' (Scott, 1988: 35). The desirability of the democratisation of higher education embedded 'as an axiom' at the core of the Robbins report was also to be embedded in the principles of the whole of society and arguments which were expressed by the UGC just ten years earlier regarding 'the pool of ability', quoted above, were made to sound utterly implausible by Robbins (cf. Anderson, 2006: 148).

II. Moving away from London and Oxbridge

A second way in which the democratisation manifested itself was the moving of students away from London and Oxbridge and towards other higher education institutions across the country: between 1938-1970s, the proportion of students at Oxbridge halved, whereas the proportion of students in universities in England, other than London and Oxbridge, doubled (Appendix I, Table 2). This is particularly significant as far as sociology is concerned, as sociology was a largely 'unwelcome' subject in Oxbridge and the only place it was being taught properly before the expansion was London. The opening of new universities and expansion of universities other than Oxbridge and London meant not only that more places were available, but that there was for the first time an opportunity for the system of teaching and for the curricula to open up to non-traditional subjects, as new universities were competing to attract the new students. There was not only physical expansion but also transformation; as the University Grants Committee reported:

> Everywhere we found evidence that the universities were regarding the years ahead not merely as a period of physical expansion but also as an opportunity to re-examine their aims and purposes' (University Grants Committee, 1964: 85-6).

We will see that it was not only the expansion but, to a large extent, the transformation of the education system that helped to transform sociology from an obscure and unpopular subject to one of the most desired and fashionable.

III. The Power of Student Demand

A third way in which the democratisation was manifested was through the power that student demand had on determining which university subjects were going to be most popular. In a recent extensive study on this topic, Mandler (2015a) argued that 'the consumer was king', showing that despite continuous efforts on the part of government and universities to encourage students to apply to the sciences, in the belief that there was a shortage of science and technology graduates, by the end of the sixties, 'the agenda for higher education was being set by teenagers and their parents' (Mandler, 2015a: 9) who were becoming increasingly more attracted to the arts and the social sciences. This created a disparity between the public discourse sustained by policy makers, which was throughout the whole post-war period dominated by an alleged insufficiency of science and technology graduates; and the discourse among students themselves which consistently emphasised the desirability of social science (for a full review, cf. Mandler, 2017).

Similar disparity, with regard to quantitative methods training, can be observed between the discourse sustained by the official reports on the state of the social sciences (Pearson, 1947; Clapham, 1946; University Grants Committee, 1964; Heyworth, 1965; Rosenbaum, 1971; ESRC, 1987) and the discourse among sociologists during this period – while the former argued consistently for more and better quantitative methods training within the social sciences and especially sociology, the latter have consistently ignored such claims or if they responded at all, rejected them. This issue will be examined further below.

The Robbins report itself did not recommend a wider and more rapid expansion of the social sciences *in particular*; and it did not anticipate how quickly their popularity would rise among the 'Arts and Social Science' students and among all students. In the whole of the Robbins report there is just one reference to the social sciences: 'More awards should be available for postgraduate study, particularly in the social sciences and the humanities' (Robbins, 1963: para. 304). The 'uppermost consideration' in the planning of the expansion in this early period was the expansion of science and technology studies, because this was seen as important in restoring and growing the economy and the general prosperity of the country (University Grants Committee, 1973: 24-5). But the rationale behind this strategy was increasingly being questioned as during the 1960s the limitations of a narrow, vocation-oriented training became a growing concern; and a report of the OECD showed that although the UK had 'the greatest concentration of science and technology in higher education and the biggest proportion of qualified scientists and technologists', the UK economy 'had been lagging, not leading' (OECD, 1950: 67).

In addition, the orientation of students towards the social sciences was not a peculiarly British phenomenon and was related to a 'disillusionment with science' observed more generally in the Western world; the Dainton report of 1968, although hesitant to describe it in either positive or negative terms, captured the presence of a subtle but pervasive cultural shift which manifested itself as 'a concern for people rather things, a move away from doing towards feeling' (Dainton quoted in Mandler, 2017: 9-10).

The statistics of higher education leading up to Robbins, and up to 1978, show a curious trend that could not have been observed at the time and could only be seen with the benefit of hindsight; a change perhaps more consequential, than 'the swing away from science'.

Separate statistics for undergraduate and postgraduate students by subject areas are not available prior to 1960. Table 3 in Appendix I contains the relevant statistics for all students, in the period 1919/20-1953/4, showing that even before the post-war expansion of higher education, the proportion of 'Arts and Social Science' students was rising; from about a third of all students in 1919, to a half by 1953/4. Since statistics on individual social science and arts subjects were not collected in this

period, it is difficult to say the expansion of which individual subjects brought about the expansion of this subject area. It is likely, however, that the increase was due mainly to an increase in the popularity of arts subjects and economics, since none of the other social science subjects were academically well developed in this period.

Meanwhile, between 1919/20 and 1953/4, there was an overall decline in the popularity of general science subjects from ca. 68% to 57% (Appendix I, Table 3). What is more interesting, however, is the trend in proportions of students within individual science subjects – we see that the decrease of science students overall is mainly due to a relative decrease in the number of medicine and applied science students, while the number of 'pure science' students increased by about 3 % points. It could be the case therefore that the rise in the popularity of 'Arts and Social Science' subjects was not necessarily at the expense of a decrease in the popularity of all science subjects, but only the more applied type, such as medicine[23] and technology. We should be careful, therefore, about what we mean when talking about students' 'disillusionment' with science.

What does this suggest for the expansion of sociology? First, it suggests that a more favourable climate for the expansion of a relatively undefined and unpopular subject like sociology was *already present*; the expansion itself only assisted the further continuation of this trend by opening the doors of universities to greater numbers of students. And second, it suggests that a subject which contained more 'pure' as opposed to 'applied' elements was more likely to benefit from this expansion, as the trend of moving to 'pure' subjects (arts, social sciences, pure science) and away from applied subjects (applied science, technology, vet science) was already gaining pace.

How did things change when the expansion began to come into effect during the 1960s and 1970s? Since from 1960 onwards there are separate statistics available for undergraduate students by subject area, the rest of the analysis focuses only the distributions of undergraduate students.

The decline in the popularity of general science subjects continued (Appendix I, Tables 4-7; also Appendix II, Figure 1). The proportion of general science

[23] It is the policy of the British Medical Association to control the number of medical students; a large part of the decrease in the relative number of medical students compared to students in other subjects is due to these control measures.

undergraduates out of the total number of undergraduates was falling consistently and in 1978/9 was down 6.9% points from what it was in 1960/1. Meanwhile, the popularity of pure science subjects observed above, continued till 1972, despite some fluctuation in the proportion of pure science students during the 1960s. Compared to 1960/1, in 1978/9 the proportion of pure science undergraduates of all undergraduates was down only 1% point. This suggests that whatever was affecting the popularity of general science in the 1960s and 1970s had negligible effect on the popularity of 'pure science' subjects. This trend is further supported by the distribution of the proportion of first entrant undergraduates – compared to 1966/1[24], in 1978/9 the proportion of general science first entrants of all first entrants was down 5.2% points; whereas in the same period, the proportion of pure science first entrants went down only 3.9% points (Appendix I, Table 7).

What, therefore, could have had an impact on the development of the particular character of sociology as it was expanding, and on its preference for some methodologies and not others, was a trend in student demand that favoured 'the pure' over 'the applied' elements in a university subject. The evidence presented here is, of course, insufficient to establish with great confidence whether this trend was taking place and how consequential it was. To achieve greater confidence in this regard, it would be necessary to examine in further detail the development of science in universities in this period which is beyond the scope of this thesis. The evidence however, suggests clearly that we have to be careful when interpreting the meaning and range of the 'swing away from science'.

Compared to 1960/1, the proportion of 'Arts and Social Science' undergraduates out of the total number was up 5.3% points; the growth in the proportion of 'Arts and Social Science' first entrants out of all first entrants shows a similar increase (Appendix I, Tables 4-7; also Appendix II, Figure 1). The big differences between the period up to 1953/4 and the period from 1960 onwards, lie in the distribution of student numbers *within* this subject group; more specifically, the rising popularity of social science as opposed to arts. We see here the impact of the cultural change that took place in the post-war period; but just as with the 'swing

[24] First-entrants statistics only became available in 1966.

away from science', we have to be careful how we interpret the 'swing towards social science'.

In the 60s and 70s, the proportion of social science undergraduates of all undergraduates grew consistently – compared to 1960/1, in 1978/9 it was up 12.6% points, making it the second biggest change in the higher education trends in this period (Appendix I, Tables 4-7). The third biggest change in the trends was the drop in the popularity of arts subjects – compared to 1960/1, in 1978/9 the proportion of arts undergraduates of the total number of undergraduates was down 7.3%. The rise of social science, therefore, meant not only a turning away from applied subjects of all kinds, but also, a turning away from the abstract non-science subjects, such as the arts.

The relevant statistics of the first entrants social science undergraduates compared to the first entrants arts undergraduates show slightly different movements which help to modify our understanding of the popularity of social science. The popularity among first entrants of *both* arts and social science grew in the period between 1966/7-1978/9 (Appendix I, Tables 6 and 7). When taken together, the proportion of all art and social science undergraduates out of all undergraduates and the proportion of all art and social science first entrants of all first entrants, suggest that there was a move from the arts to social science *after the first year* – unless of course these results are an artefact of the way the statistics were collected, this appears to be the most plausible way to explain why the total number of arts students decreased over time, despite the fact that they enjoyed an increase in the number of first entrants over the 60s and 70s. This provides further support for the idea that even though the arts did not lose their lustre as first choice, eventually, many students were drawn to the more relevant and more fashionable social sciences.

Further confirmation comes from the trends in the proportions of arts as opposed to social science undergraduates of all 'Arts and social science' undergraduates (Appendix I, Tables 4-7). Here we see the biggest change in the higher education trends – whereas in 1960/1 only 26.8% of 'Arts and social science' students studied social science, in 1978/9, their proportion rose to 50.3%; an increase

of 23.5%[25] points. But, the proportion of first-entrant social science students of first entrant 'Arts and social science students', despite some fluctuation, dropped (Appendix I, Tables 6 and 7). This means that although many students were not initially attracted to social science, perhaps thinking that arts is a safer bet, many of them moved from arts to social science, increasing the overall proportion of social science students of 'Arts and social science' student in all years.

What, then, could have made the higher education context, leading up to and during the higher education expansion, favourable to a subject like sociology – a subject that, as we have already seen, was vaguely defined and therefore had the potential to develop in more than one direction in regard to its substantive choices and methodological approach? To talk about a swing away from science and towards social science is to tell only half the story. The analysis of the statistics of education for the period 1919-1978 presented above is by no means conclusive; but it suggests a move away from subjects with pronounced applied and less socially relevant elements towards subjects with pronounced 'pure' and socially relevant elements – sociology could have benefited, and indeed did benefit, from developing into a subject that emphasised the 'pure and socially relevant' elements at the expense of 'applied and less socially relevant'. 'Pure' science did not lose its staunch supporters; but its popularity was nonetheless affected by the social sciences because they spoke about issues which were highly relevant to the socially active generation of the 60s and 70s. But applied science and the less relevant non-science subjects, suffered in this context. It was these conditions, as far as higher education is concerned, that could help explain not only why sociology could expand, but also why it did expand as, primarily, a non-empirical subject emphasising the development of theory which spoke about society with an irresistible charm.

This tendency was not a sudden shock to the university system – it was well anticipated in the late 1950s; the mere fact that the UGC began collecting separate statistics for 'Arts' and 'Social Sciences' in 1959 (the first such statistics published by UGC in 1960/1) is a clear sign that from the late 50s onwards, social sciences

[25] The popularity of social science within the context of 'Arts and social science' fluctuated considerably during the 60s and the 70s; they were, in fact, most popular in 1966 when 51.2% of all 'Arts and Social science' undergraduates studied social science.

were being taken much more seriously than before. A UGC survey of universities confirmed the importance of this trend:

> Within the category of arts and social studies we felt that there was certain to be an increasing demand for a substantial development of teaching and research in social studies. Their growth is of particular importance not only in their own right but as an important component in the education of scientists and technologists. Our view was shared by the universities. We were encouraged to find the universities aware of the need for increased development in this field and this was reflected in their development plans for the 1962-7 quinquennium. Most of them have recognized its growing importance by the establishment of separate faculty of social studies or social sciences. Progress has been hampered […] by the shortage of qualified teachers and research workers (University Grants Committee, 1964: 82).

As the survey suggested, the peak in the popularity of social sciences occurred in the mid 60s, just after Robbins. It is worth noting that the social sciences were considered valuable not merely in their own right but also as an important component in the education of pure and applied scientists; a component that could 'liberalise' their education and broaden their intellectual horizons. The UGC believed that social science could indeed help in establishing a communication between the 'arts' and the 'sciences':

> We think that the role of the social sciences as an undergraduate discipline has an important role to play in the expansion lying ahead; we think, too, that they are a particularly important link between the pure and the applied sciences and the arts […] (University Grants Committee, 1964: 160-1).

The UGC therefore welcomed 'the growing introduction of the social sciences into engineering curricula' (University Grants Committee, 1964: 161). It is noticeable, however, that the communication was flowing one-way – the scientists were receiving 'general education' through social scientific courses but there was no sign that the 'arts' students were receiving courses in science. The universities in general were prepared for the arrival of the social sciences – the UGC considered that their development 'would be stimulated by the separation of this field of study from the other arts' and suggested to the universities 'that they might establish separate faculties of social studies where these did not exist already' (University

Grants Committee, 1964: 161). But, they were 'glad to note that most universities now have such faculties (University Grants Committee, 1964: 161). Sociology could not have hoped for more favourable circumstances and, as I discuss later, given the roles that the social sciences as whole were perceived to fulfil, sociology turned out to have particular advantages compared to other social science subjects.

IV. Demographic Characteristics of Incoming Students

A forth way in which democratisation manifested itself was with regard to the demographic characteristics of the new generation of students. As Mandler (2017) argues, the expansion allowed for less academically qualified pupils who came from families with no previous experience in higher education to enter university and these students were more likely to see the social sciences as more attractive because they were perceived to be less difficult. So, in addition to being perceived, at least culturally, as more relevant to the *zeitgeist* of the 60s and 70s, the social sciences were also being seen as a safer bet on the part of students for whom higher education was an unusual and risky step. Another important change was the growing number of girls entering higher education, among whom the views I outlined above were more prevalent. As I show below, the increased participation of girls in higher education had an important role to play in making possible the expansion of sociology and in sustaining its popularity without increasing the requirements of quantitative skills for sociology students.

V. Labour Market Opportunities

And finally, a fifth way in which the democratisation manifested itself was in terms of the labour market available to graduate students. The opportunities for jobs and the variety of jobs encouraged many students to pursue the study of subjects which were not vocational or leading straight into a particular job. The change was especially noticeable with regard to the increase in opportunities for work within the

public sector, making it possible for students 'to swing away from science without losing ground in the labour market' (Mandler, 2017: 12); but also, as I showed, to move away from applied subjects of all sorts. This was another factor in strengthening the position of sociology, making it less important that the subject itself, being still quite underdeveloped academically, could not offer any particular job guarantees to students.

Mapping the Expansion of Sociology

This section summarises the evidence for the expansion of sociology within the social sciences and within higher education as a whole in attempt to fill a gap in the scholarship which so far has consisted, primarily, of sketchy and partial accounts. Four different areas are considered – the expansion of students, of sociology university teachers, of sociology departments, of sociology research. Collecting statistics on the expansion of sociology is a challenging task – official statistics on sociology are not available prior to 1966. Most of the following analysis is based on official sources; certain details, however, were only available from secondary sources, such as contemporary accounts on the expansion of sociology, or reports on various topics which discuss the expansion in addition to their main topic. Most of these secondary sources, however, very often fail to properly reference the sources of the figures they quote; the reader is urged, therefore, to treat the figures coming from non-official secondary sources with caution.

I. Expansion of Sociology Students

Separate statistics for sociology students are available only from 1966 onwards and so it is difficult to map the expansion of sociology properly before 1966. The only reliable source of evidence for the expansion of sociology prior to 1966 comes from the Heyworth report (1965). Heyworth shows a consistent large increase in the numbers of students obtaining honours degrees in social science – in 1938/9 only 33

students obtained honours degrees in sociology, social administration and anthropology; by 1962/3, this has risen to 341 (Appendix I, Tables 8 and 9). The number of students obtaining honours degrees in economics also grew – 216 in 1938/9, compared to 994 in 1962/3. However, the absolute growth of economics degrees offers a rather biased image of the growth of its popularity, because *relative* to sociology its popularity dropped. While in 1938/9 only 11% of all social science degrees were awarded to students in the sociology group, by 1962/3, this had increased to 22%. In contrast, while in 1938/9, 72% of all social science degrees were awarded to students in economics, by 1962/3, this had decreased to 64.7%. Clearly the social sciences were expanding even before Heyworth's report; but the trend between 1938/9-1962/3 was more pronounced in the sociology group of subjects than in any other social science subject. Already in 1965, when the SSRC was established, the *Times* reported:

> University teaching and research posts in the social sciences are expanding more rapidly than those of any other faculty and the number of undergraduates reading them is growing faster than the total number of graduates (Unknown Author, 1965: 13).

This trend, however, continued only till about the mid 70s (Appendix I, Table 5). Between 1966/7 and 1973/4 the proportion of sociology undergraduates of social science undergraduates and of all undergraduates was growing, despite some fluctuation; but from 1974/5 to 1978/9 we see a gradual decrease to a level slightly below the proportions of 1966. The trend is further supported by proportions of sociology first entrants of social science entrants and of all first entrants – this was growing from 1966 to about 1973 and then dropped to level slightly lower than that of 1966. In contrast, the proportions of economics undergraduates of social science undergraduates and of all undergraduates and the proportions of economics first entrants of social science first entrants and all first entrants, were consistently growing in the period 1966/7-1978/9 (Appendix I, Tables 5-7). The increases are not large but signal a greater level of consistency in the popularity of economics. On the whole, however, sociology still appears to be as one of the subjects that benefited most from the expansion precisely because its share in the universities before 1938/9 was virtually zero. No other subject began its expansion from such a low level.

II. Expansion of Sociology Teachers

Just like the expansion of sociology students, the expansion of sociology staff is difficult to assess because there is little information available in the official statistics records. From what is available, however, we see that, overall, staff trends mirror student trends; what's more, staff statistics provide an even stronger indication of the rise in popularity of the social sciences.

When the Clapham committee reported in 1946, 'there were some 52 of these posts [full-time professors and readers] in the social sciences whereas in pure science there were 296 and 176 in medicine' (Clapham, 1946: para.14). But the social science staff of all ranks, not just professors and readers, was not in the minority for long. In 1954, Banks reported that as a result of the Clapham recommendations, 'the number of university teachers and research assistants of all grades in the social sciences had increased from 291 in 1937-8 to 604 in 1949-50' (Banks, 1954a: 49). These were not all sociology posts, but it is likely, having seen the student trends for this period, the majority of these posts went to sociology and economics – the first rising faster and more pervasively than any other social science; while the latter kept its strong position (Appendix I, Table 8). A couple of years later, the expansion of social science staff was further confirmed by Heyworth who reported that between 1938/9 and 1962/3 'the number of university teachers of all grades in the social sciences increased fivefold from 212 to 1025 (Heyworth, 1965: 8). In 1975, Smith (1982: 150) noted that Heyworth's 1962/3 figure of 1025 had risen to about 7000.

To put all this into perspective, while in the 60s and 70s the proportions of arts, pure science and general science staff all declined, the number of social science staff was rising both as a proportion of all staff and as a proportion of 'Arts and science' staff (Appendix I, Tables 10 and 11). The fact that the decline in staff numbers in general science and in pure science was highest, and that the rise in social science staff was also highest helps us further explain why such a big part of the discourses taking place in the 60s and 70s were oriented towards the dichotomy of science vs. social science. Perhaps it also partially explains why the rise of the social sciences, although fashionable and exciting among students, was perceived as a threat to science in some academic and policy circles.

But in other circles, the students' excitement about sociology was, above all, a big challenge. Martin Albrow, a president of the BSA in 1986 recalled that when sociology was expanding in the 1960s, university vice-chancellors 'stood in their doorways like hungry restaurateurs offering chairs to those who would enter and deign to call themselves sociologists' (Albrow, 1986: 337). With these words Albrow summed up not only the facts of the expansion, but also the major challenge for sociology – a lack of trained sociologists.

III. Expansion of Sociology Departments

The expansion of social science university departments was a direct effect of the growing student demand for these subjects (Appendix I, Table 12). In 1945, there was only one university in Britain – the University of London (LSE) – teaching sociology for a first degree. By 1962, there were ten universities in Britain teaching a first degree in sociology (Fincham, 1975: 123). By 1989, this figure had risen to forty-five (Review Committee on Sociology, 1989: 15).

Alongside the rise in sociology first degrees, sociology courses were also becoming increasingly popular. In 1945, such courses were taught only at the LSE and Bedford College. By 1954, between 11 and 14 universities across the country were teaching sociology courses, among them Birmingham, Leeds, Liverpool, Sheffield, Edinburgh, Glasgow, Aberystwyth, Exeter, Hull, Leicester, Swansea (Banks, 1954a: 49; MacRae, 1953). A report by Peel (1968) on sociology methods and theory courses collected data from 28 universities which taught sociology at an undergraduate level; and by the time of the next, comparable report in 1979 (cf. Wakeford, 1979) the number was 77 universities teaching sociology courses (only some of these taught sociology at postgraduate level).

The expansion of sociology teaching would not have been possible without the establishment of social science departments in the new universities or expansion within the old. But there is some evidence to suggest that the expansion of sociology teaching was more strongly pronounced than that of any other social science subject: during the 50s and early 60s, sociology had grown with 'explosive force from two or

three centres to practically every university, including the new universities and the colleges of advanced technology' (Heyworth, 1965: 11)

IV. Expansion of Sociology Research Output

The subject of sociology grew not only institutionally but also in terms of intellectual output. An important change was the establishment of a Social Science Research Council in 1965 (cf. Heyworth, 1965; Nicol, 2000) opening opportunities for many new funded research posts. Various extensive accounts of the development of new sociological research areas and the expansion of already existing ones are available (Madge, 1957; Little, 1963; Krausz, 1969) and there is no need to summarise them here. For an overview of the broader picture, it is enough to point out that the number of sociology projects increased threefold between 1945 and 1967: there were 132 in 1945-60; 295 in 1961-66; and 340 in 1967 (Carter, 1968: 18). Social science monographs more generally increased at a similar pace: while there were 2774 social science monographs in 1950; in 1960 there were 5444; and in 1970 – 9392 (Stewart, 1989: 179). Summing up the expansion of social science research, Stewart commented:

> What had happened in three phases over a period of forty years in science was covered in ten to fifteen years in the social sciences, which had, besides anything else, to discover an identity as an extended university faculty (Stewart, 1989: 179).

We see that as we move from the expansion of student numbers, to teachers, to departments and intellectual output, there is an accumulation of doubts and a greater awareness of the challenges facing sociology. Behind Stewart's comment lie fundamental questions: could sociology achieve academically in ten to fifteen years what science achieved over a period of more than forty years? Could sociology meet the challenge of the expansion and fulfil expectations?

Now that we have a clearer view of the historical development of sociology prior to the expansion and have also reviewed the higher education context of the expansion, as well as the expansion of sociology itself, lets proceed to examine the

expansion from within – how the democratisation of education and the five ways in which this was manifested affected the way sociology thought of itself; how the intersection between the two historical contexts created conditions for the development of some types of methods training in sociology but not others. It will become clear that sociology's biggest weakness – its lack of a clear sense of direction and limited achievement up to 1945 and its vague but nonetheless potent character – proved sociology's greatest strength once the democratisation of education took place. Sociology could easily *turn* into what others perceived it to be, or what sociologists perceived sociology *had* to be in order to expand. But there was little in the conditions of the expansion that stimulated the incorporation of statistical methods or, more broadly, a statistical outlook among sociologists.

Characteristics of Academic Sociology during the Expansion

It is one thing to explain what conditions made the expansion of sociology practically possible – in our case this was this was primarily the expansion of the university system and the establishment of the SSRC in 1965 – and a completely different thing to explain what characteristics of *sociology*, as opposed to any other social science subject, helped it to take advantage of these practical opportunities.

To explain this we need to take into account the broader cultural context which accompanied the expansion that was influencing people's thinking about education. As I already mentioned the first way in which democratisation of the higher education system manifested itself, was through a change in its core principles – education was not to be a privilege for the upper classes, but a right for all. But together with the implementation of this principle, another very important and closely related aspect of the understanding of higher education also changed – namely, how students thought about *their own* education; what was education *for*?

In 1927, H. E. Barnes, an American scholar, commented on the contemporary higher education system in the UK, describing it as 'medieval' and 'humanistic'; aimed to train 'the cultured gentleman in terms of the older criteria of "culture" and 'to prepare the university man to move easily and urbanely in formal social circles'

(Barnes, 1927: 46). This could no longer be the case once higher education was, at least in principle, supposed to reach all, regardless of their social status or class. So the purpose of education in the post-war period changed from preparing 'cultured gentlemen' to being part and parcel of attempts to understand social processes and tackle the most pressing social problems. Such attempts had been developing consistently since the early nineteenth century; but it was only in the post-war period, as a result changes in the core principles of education, that higher education came to be at the centre of these attempts. In this way, the change of priorities and of principles guiding higher education were an invitation to sociology – how could education be *for* society as well as *about* it, without a discipline whose object of study was exactly that – society? The fact that new universities were being set up made the process of re-orientation and implementation of these new principles in the higher education system easier and it was these newer universities that gradually became sociology hubs. So; if the principles behind the education system had changed and were being implemented in the newly established and expanding universities where the majority of students were, what was it that made it possible for sociology to meet the student demand, what helped sociology *fit* the new education principles?

So Attractive, so Quickly

The British Sociological Association (BSA)[26] was established in 1951 as an organisation whose scope was deliberately 'made very wide in order to bring all those who are interested in the sociological aspects of their special subjects' (Banks, 1967: 1). The people who signed the declaration establishing the BSA were people from a wide variety of backgrounds, such as demography, archaeology, social anthropology, social philosophy, economics and social psychology. Barbara Wootton, who was one of the people in the founding group, was reported to have given the

[26]The BSA and the SSRC/ESRC both played an important role in the post-war development of sociology; however, a proper investigation of the role of these two large organisations in the relationship between sociology and statistics would have required more time that I had available and has been left for a future study.

following comments regarding the eclectic approach employed at the founding of the BSA: 'And none of you are proper sociologists. Look around you. You and you and you, none of you has ever studied sociology! You've all come in from somewhere else!' (Oakley, 2011: 165). Wootton was, of course, right, and was stating something that Joe Banks continued talking about fifteen years later. But what matters historically is not the fact that the BSA was founded by a group of scholars all coming 'from somewhere else', none of whom were 'proper sociologists'; what is important, is that *none of them could have been* 'proper sociologists', unless they had studied at the small Sociology department at the LSE or abroad, say, in the USA. How then could people be attracted to a subject that did not exist in Britain in a well-defined and well-organised way? And how did the expansion of higher education turn into a golden opportunity when, given the history of sociology in the UK in the nineteenth and early twentieth century, it should have been sociology's greatest challenge?

The BSA meeting was a microcosm that was reflected in the state of the whole subject of sociology. The students, graduates and academics who were drawn to sociology in the early period of the expansion, did not all come from the same background, or for the same reasons, or with the same intellectual interest and were not all looking to get the same thing out of sociology. This made sociology very unusual compared to other subjects which did not usually accommodate such a great diversity of people. It was not as if sociology catered for one specific and predefined group of intellectuals or students or those who only got the opportunity to enter higher education as a result of the expansion. The education statistics discussed above show a trend of students moving away from applied scientific subjects to more 'pure' and socially relevant subjects. But the real power of sociology was that it attracted students and academics from a variety of other subjects, who were disillusioned with these subjects, or who wanted a change for one reason or another and who were drawn to sociology expecting that it would provide them with better opportunities to understand and engage with the social world. How could sociology do this?

For most of the period leading up to the expansion, and even during and after it, sociology was and remained a subject that was not clearly defined. At first sight, this

may look as a disadvantage, but on closer inspection we see that the subject benefited from this, mainly in the sense that it could easily adapt to the circumstances and offer what was in demand. Sociology could easily turn into the subject that others believed it to be or wanted it to be.

Starting first with the public status of sociology, it was not uncommon during the 1960s to hear sociologists argue that sociology 'has become an accepted constituent in the teaching and research of British universities' (MacRae, 1957: 98) and that sociology was 'a word of virtue and of power' (Little, 1963: 64). But despite the fact that this enhanced feeling of security had some justification, the difficulty remained that sociology was 'the discipline which people find most puzzling of the major social sciences' (Heyworth, 1965: 3); perhaps even 'a chimaera, disconsolate on account of its incompatibilities' (MacRae, 1964: 24). Popular accounts of sociology did not lack in 'mordant humour' either – in an attempt to explain the difference between sociology and history, it had been claimed that 'sociology is history with the hard work left out: history is sociology with the brains left out' (Briggs, 1965: 26). But the bottom line of all these comments, be they serious or humorous, is that the subject of sociology 'had acquired general respect' and 'received widespread recognition', *even though 'the public generally are not clear what it is all about'* (Banks, 1965: 109, *my italics*).

But the lack of clarity about what sociology *was* continued during the expansion period even among sociologists themselves. Their struggle to define sociology in the context of its rising popularity within the universities and among students is nowhere as clear as it is in their attempts to define sociology in sociology textbooks and guides for students. Few of the attempts that were made by the first generation of professors of sociology in the 60s went beyond re-stating the general definitions of sociology that had been around since the nineteenth century – Tom Bottomore, for instance, author of one of the first British post-war textbooks of sociology defined sociology as the first science, together with social anthropology 'to be concerned with social life as whole' (Bottomore, 1962: 20). Another famous textbook, by Stephen Cotgrove, defined sociology as a social science that 'places more emphasis on 'how' questions and on building general theories of social systems' (Cotgrove, 1978 [1967]: 40). In his inaugural lecture as a Professor of Sociology at

the University of Edinburgh, Tom Burns, went even as far as to question the extent to which sociology can be defined without reference to other core subjects:

> […] substantive fields of sociology, many of them at least, go by titles like the sociology of education, the sociology of law, the sociology of politics, the sociology of medicine. In all these cases, the substantive area of study is defined by another discipline. The mapping has been done by it, not by sociology (Burns, 1967: 356).

But even in the light of such evidence, it can be argued that we cannot judge the clarity of a subject based on its lack of success in defining its own subject matter in a couple of sentences, and that one needs to read the whole textbooks to find out what sociology was all about. But this does not mean that the problem of making sociology clear, especially to students, did not exist. Here is an attempt to describe the subject in the *LSE Handbook of Undergraduate Studies* of 1965-6:

> Sociology is in some ways difficult subject to describe. It is concerned more with the private relations of people, the kind of things that are not organised by governments […] The sociologist and the social anthropologist spend a lot of time describing and analysing family relationships and family organisations. They are concerned also with all the other ways in which people group themselves […] One object of the sociologist's study which is familiar to most people in this country is the class structure of our society […] The sociologist is also concerned with the organisation of social welfare services […] The sociologist tends to be led on the study of psychology and the study of philosophy (LSE, 1965: n.p.).

This meandering description is a confirmation that even in the setting of undergraduate teaching only approximation to clarity as regards definitions was possible; and that lack of clarity was, in fact, acceptable. John Rex, author of one of the first British textbooks on sociological theory had reached a similar conclusion:

> It is obvious then that we cannot conclude […] that there is a clear-cut subject matter for sociology which can be settled by some sort of ostensive definition. […] it is obvious that the definition of the field of sociology depends above all on the attainment of theoretical clarity (Rex, 1961: 58-59).

What all these examples show, is not merely the existence of confusion about sociology. More importantly, they show that although clarity as regards substantive topics and methodology was lacking, people also felt this type of clarity *was not a necessary condition for the progress of sociology.* It appears to have been enough to *believe* in sociology; to believe that sociology was about *ideas* that mattered *to them* – sociologists, sociology students, the general public. So for instance, with regard to the public, Albrow admitted that 'more than any other discipline sociology represented that faith [in the re-organisation of education and social progress] for the educated public (Albrow, 1989: 203) and that sociology acquired its place 'not primarily on the basis of intellectual argument, but because it was summoned up in an act of *faith'* (Albrow, 1986: 336, *my italics*). It was 'a novel set of circumstances that forced sociology in front of the public', Albrow continued, and not 'the sheer force of argument and demonstration of results' (Albrow, 1989: 202). And with regard to the students, when Banks conducted a survey with 1966 sociology graduates, he found that:

> Excellence in sociological attainment, it would seem, is related in some marked fashion to an interest in ideas and it is therefore likely that a graduate's orientation towards sociology as a vocation is linked in his mind with the extent to which he finds working with ideas more attractive or congenial than a practical encounter with the world of events and personalities (Banks, 1974: 300).

And, as for the sociology teachers, Bechhofer recalled that they had 'a missionary zeal'; that they believed there should be more sociology not because they thought there should be more *sociologists per se* but because sociology prepared students to work in business, management, civil service better than, say, history or English literature (Author's Interview with Frank Bechhofer, 2017). Sociology, it was believed, was providing a better platform for the discussion of the most relevant ideas about society.

But this would have been the case in many countries in Europe and also in the USA. The spread of sociology in Britain would not have been much different from the spread of sociology in other countries had it not been for the fact that sociology was felt to be an 'appropriate intellectual discipline for the new problems of the

welfare state […] *irrespective of what was on offer'* (Albrow, 1989: 202). Previous chapters have shown that sociology in Britain before the expansion did not, in fact, have that much to offer.

But apart from having the reputation of a subject in which people believed, what gave sociology greater power was the fact that the lack of a clear definition meant that the ideas of students and of teachers about what the subject was could be projected onto the subject without any restraint from 'a sociological authority'; projecting characteristics which one could not project on to other subjects with traditional approaches and fixed boundaries. It was in this way that the vagueness of sociology was turned from an internal problem into an advantage. So for instance, in the 60s sociology appeared to be offering novel approaches to understanding society and more interesting problems, compared to, say, history:

> What attracted me was that sociology seemed to offer up some interesting problems, like the relationship between Protestantism and capitalism; […] What put me off history was - I mean these people were superb scholars, they had great mastery of technique which I admired but it was not clear to me where they got their problems from and the problems they came up with were not very interesting […] sociology seemed to be asking more interesting questions than conventional history (Author's Interview with John H. Goldthorpe, 2017).

But, Goldthorpe continued, he 'could not have been able at that point to say anything very general about sociology at all', which tells us that the image of sociology offering up more interesting problems than history was one that he himself projected onto sociology even before he came to study the subject.

Or one could study engineering but instead of developing further one's interests in the engineering profession, one could take up 'industrial sociology' and study instead how the engineering profession was organised, how innovation was brought about and implemented in a variety of industries (cf. Author's Interview with Frank Bechhofer, 2017). It was similar if one was training to be a teacher – in addition to classes on pedagogy, one would take a course on sociology of education that dealt with broader issues that permeated the whole education system, like social class and social mobility and find them more fruitful (cf. Author's Interview with Robert Burgess, 2017).

Sociology also offered an opportunity to reconsider the content and role of existing subjects, as for instance the new branch of sociology called sociology of science was able to attract science students from science faculties, not because there was something wrong with science, but because this was a novel aspect of the production of scientific knowledge that attracted students' attention (cf. Author's Interview with Donald MacKenzie, 2017).

What we learn from examples, such as those presented here, is that there was not something wrong with subjects like history or engineering or science; rather, there were new opportunities for students who had considered studying these subjects or who studied these subjects and wanted to change, to find a more suitable outlet for their more immediate concerns stemming from these subjects. Sociology offered an opportunity to examine topics related to, or part of, other subjects, from a different angle or in a more interesting way.

So far I have explained how it was possible for students to project certain ideas onto sociology and why it, as opposed to various other academic subjects, was better suited for this purpose. But I have not spoken about the particular kinds of ideas that students wished to have addressed in their education and also where they, and the students' desire to address them, came from. For this purpose, we have to look at the broader – social and political – context which changed during the period of the expansion. The raised social and political awareness that we witness during this period explains why it was some ideas and not others that sociology became associated with in this particular period. The association of sociology with particular ideas gives us direct clues as to what the obstacles were for the incorporation of statistical methods and statistical thinking into sociology.

Relevant

The cultural, social and political change taking place during the 60s in Britain involved aspirations to liberate people from the old and conservative ways of thinking about society. There was a strong desire to bring about big changes in social welfare and social norms, to emancipate citizens and revolutionise fashion, music,

literature, film etc. Sociology offered space for discussion of all the relevant questions about society that young people were thinking about; it appeared to provide the intellectual support for the aspirational hopes for change in society. The expansion of sociology in the higher education system put sociology in a strong position to take advantage of the demand being created for knowledge of things social.

Contemporary attempts to explain what had triggered the 'sociology boom' were limited in their scope but even this is revealing. A newly established sociology journal at the time, *New Society*, observed that a feasible explanation for the rise of sociology was 'the evident need, with increasing change, for society and its institutions to have some system of criticism built into them' which the social sciences were believed to provide (Unknown, 1963: 3). Another way of explaining the sociology boom was an apparent increasing uncertainty which many people felt about their relations with each other, so sociology was seen as being able to help the young generation cope in new situations. What is peculiar about such descriptions of the functions that sociology was supposed to fulfil is, first, their ahistorical viewpoint – both of these things, but especially the function of providing criticism of society, could just as easily been said about social statistics in the nineteenth century; and second, the complete lack of detailed discussion as to how exactly a subject like sociology was going to do that. We see so little discussion about 'how questions' - questions which are essentially about strategy and methodology – that one cannot help think about the expansion of sociology as similar to the spread of some all-healing religious movement; a 'cult' even, as *The Times* described it in 1970 (Unknown Author, 1970: 17).

The explanations of sociology's sudden expansion that followed in later decades continued emphasising that the subject's rise was primarily a matter of providing a fresh platform for the discussion of society's most prevalent problems, leading to the formation of a variety of social movements. The feature that was conspicuous in all social movements (campaigns for nuclear disarmament, student radicalisation, liberation of the young, unrest among the working classes, commercialisation) was the rejection of authority claimed by the established institutions (Dennis, 1989). Alongside this, there were specifically social-political

factors, such as the rise of the social welfare profession, the post-war socialist legislation of the Labour party, questioning the basic assumptions of the class structure, community awareness, recognition of societal problems etc. (Owen, 1979: 93). The urgency to engage with these issues drew the attention of young people and academics to sociology:

> Sociology in the USA and Western Europe […] seemed for a time and to many to be the great source of Enlightenment on the conditions that actually existed in urban-industrial society. That was why departments of sociology were able to be established in the first place, why students were attracted to them, and why able people chose to make sociology their profession (Dennis, 1989: 420).

Knowing that sociology had already developed in the US along similar lines also meant that a justification or elaborate explanation of why sociology was getting so popular so quickly was not necessary. In a recent interview, Donald MacKenzie recalled that:

> one did not have to explain it or justify it. It had happened. Social sciences were seen as important and probably relatively novel. People were aware that the social sciences, particularly sociology, had developed far further in the United States than in the UK (Author's Interview with Donald Mackenzie, 2017).

Once the process of expansion had begun, 'Why sociology?' or 'How did you become interested in sociology? became very quickly, as Jennifer Platt recalled, 'silly questions' because 'everybody got into sociology then!'; even to this day, those who were students in the early stages of the expansion argue that they 'just thought that was normal'; 'it was the height of fashion' (Author's Interview with Jennifer Platt, 2017).

The problematics of methodological training was not an issue back then; many of the first new generation of post-war sociologists did not receive formal training in methods; focus was elsewhere. Sociology spoke to emotions and hopes, rather than career ambitions or the acquisition of technical skills. In a recent interview, John Wakeford recalled that sociology was 'new, it was radical, it questioned things' (Author's Interview with John Wakeford, 2017). And while the public and the media

were sceptical or even hostile to sociology, not seeing it as 'a proper subject', for students applying to study sociology it was an act of rebellion – 'the public thought it was a waste of time but the rebellious teenagers thought "That's just what I want to do then!" (Author's Interview with John Wakeford, 2017). Using one's education as an act of rebellion was facilitated by the fact that:

> one of the things about going to university in those days, was that you did not necessarily expect to get anything out of it […] If you were really interested in sociology […] well, you went and did a degree in it. And it did not have to lead anywhere because if you've got your degree, well you could go out and get a job (Author's Interview with Robert Moore, 2017).

But, most importantly, this was not a rebellion against all of society but rather against its established structures, including established educational and career paths. During the expansion, 'among the original interests and motivations with which students come to the study of sociology [is] the prevalence of a desire to remedy social ills here and now, to "work with people" or "to help people" (Neustadt, 1967 quoted in Fincham, 1975: 175). Broady (1967: 408) also reported that many of his students believed that sociology would tell them how to make the world a better place and relieve people from distress; sociology was about 'nice things' such as families and 'good things' such as communities. The time of the expansion was seen as a time of unlimited opportunities – not only was the subject of sociology unconstrained by particular methods or older traditions; not only were universities answering the students' demand for it unquestioningly, but there was little fear that sociology could fail students because of an expanded labour market. Wakeford summed up the emotional appeal of sociology as follows:

> Students loved it because it was…you know…it got away from the stuffy classics and science and philosophy; it was real, it was immediate, it was addressing real problems, social problems, political problems – so it became very popular (Author's Interview with John Wakeford, 2017).

What is the significance of this emotional appeal, of the image that sociology quickly created for itself, for sociology's relationship with statistical methods and statistical thinking? At the very least it helps us see more clearly the way people, especially students, thought about sociology at the time and understand why anything

that was old and 'stuffy', regardless of how well established it was or how methodologically rigorous it was, *could* be rejected. In this social and cultural climate, and without facing any restrictions from within sociology, students 'placed moral evaluation before empirical analysis' (Broady, 1967: 408). There is no indication that, at the time, sociologists were, or perhaps even could have, made it clear to their students how the ability to handle adequately statistical data could help them do good for their society. As far as the students were concerned, statistics, like history, was seen as one of the old and stuffy subjects which were out of tune with the emotions of the time.

Active

But sociology's popularity in the post-war cultural context was not just a matter of relevance or emotional appeal. Sociology was not perceived merely as a passive study of society but as a commitment to active engagement with social issues; not just a source for Enlightenment ideas, but enlightened *action* aimed at changing society. Helen Roberts, for instance, recalled that people were hopeful that social science 'would have something to tell us about how societies might operate in a different kind of way'; 'it was all about how […] people could change the world' (Author's Interview with Helen Roberts, 2017). These hopes were further fuelled by the fact that in this 'progressive era' there were a lot of new actual opportunities created to achieve a change for the better, manifested in the expansion of the welfare state and civil service (Author's Interview with Robert Moore, 2017). The aspiration towards social activism that sociology fed into was also mixed with political activism – sociology was the new politics. There was a common joke, Robert Moore recalled, that 'if you were interested in politics, you did sociology, and if you were not, you did politics' (Author's Interview with Robert Moore, 2017). There was a common understanding that sociology was a subject in which one could use both one's intellect and one's ideology (Author's Interview with Helen Roberts, 2017) – sociology was not just a matter of understanding the social world, it was also a matter of changing it, using the momentum of progressive politics. Thus, many

young people at the time 'had got hold of the popular image that it was somehow either very socialist or very socially progressive and this is where they wanted to be' (Author's Interview with Robert Moore, 2017). At the same time, there was little to suggest that in order to act upon the social and political principles that students upheld, they had to possess empirical skills for doing social enquiry. In a section below I explain in greater detail how the common ideas about sociology clashed with the common ideas about statistical methods; but for the moment it suffices to say that sociology and statistics appear to have occupied the opposing ends of the political spectrum – while, generally speaking, sociology was associated with progressive politics, there was a widespread suspicion that quantitative methods were 'in a sense reactionary' and therefore of no great use to those who were working on sociological problems (Author's Interview with Donald MacKenzie, 2017)

Theoretical

Finally, there was one other aspect of students' and teachers' understanding of sociology during the expansion that helped define the subject in this period. This was the idea that sociology was primarily a theoretical enterprise – that the relevant, immediate and political concerns that were at the forefront of students' and teachers' minds were supposed to be addressed using new radical and innovative theories. John Wakeford for instance, remembers that 'social research methods was a low status activity' and that the 'high status activity was pure theory, abstract theory' (Author's Interview with John Wakeford, 2017). This kind of perception influenced what types of research or teaching were seen as appropriate. Rosemary Deem, who was an undergraduate student in the 1960s recalled that 'one could not "get by" in sociology simply by reading nice little empirical studies' and that 'theory which [she] often found incomprehensible was the prestige' (Deem in Banks, Deem and Earnshaw, 1980: 72). As a result, for those who thought of themselves as sociologists 'to go out there…out in the field was not something that anybody wanted to do' (Author's Interview with John Wakeford, 2017). Those, who did 'go out' like Deem, had their work being described as 'antiseptic sociology reminiscent of a previous era'

(Deem in Banks, Deem and Earnshaw, 1980: 73) or unworthy of attention as in Wakeford's case: after delivering a paper on methods at a BSA conference, Wakeford was not given a chance to receive questions because the chair, who 'obviously thought [that Wakeford's topic] was totally uninteresting', urged people to break for tea (Author's Interview with John Wakeford, 2017). Contemporary accounts also confirm the perception of theory building, and theory building alone, not in combination with methodological training, as the superior activity: 'sociology is first of all concerned with the scholarly endeavour to establish a general theory of societies, and that this is our primary duty' (MacRae, 1964: 24). The late 50s and early 60s saw the publication of a variety of books, including those of Peter Winch, John Rex and W. G. Baldamus which in different ways emphasised 'the importance of the activity of "theorising" in its own right' (Rex, 1966a: 529). In more general terms, there was a belief that 'words are primary' because they 'can suggest finer shades of doubt or affirmation than any measure of significance' (MacRae, 1969: 601). It may be argued that methodological training and development of methodological techniques is implicit in such statements, but the fact is that this was rarely mentioned, and even if it was mentioned, details on how a coordination of theory and methods would be achieved were lacking – all of which signifies that at this vital period of the expansion the focus was largely, if not exclusively, on theory.

By defining sociology as a subject that aims primarily at theory building, early sociologists (the majority of whom did not have training in empirical methods, let alone training in quantitative skills) were able to justify their own place in sociology departments and their own abilities; but, perhaps even more significantly, they raised up a generation of new young sociologists, that would eventually come to dominate the subject during the post-war period, who also lacked such skills and who saw no value in possessing such skills. This second generation of sociologists:

> at the start of their career were the students of people who knew no quantitative methods. They were given a vision and a conception of sociology, in which quantitative methods at best were ignored and at worst were decried. They have made their careers this way and it's just a continuity in their work…(Author's Interview with John H. Goldthorpe, 2017).

A similar orientation towards theory – seeing theory as something necessary but also as something that made sociology attractive – was also a factor in the changes that took place in social science and administration teaching. An examination of the development of the subject in the next chapter shows that the subject never lacked a theoretical part, albeit small. It has even been claimed that it was this theoretical facet of the subject that helped social science and administration expand in the early post war period:

> It was the emergence of a second facet of the subject, which, along with vocational need, resulted in the expansion of undergraduate courses. The scale and complexity of social provision has fostered a drive towards policy analysis, theory building and cross-national comparative studies which in turn made the subject 'more attractive' not only to students who wanted to pursue careers in the social policy and administration field, but more broadly (Review Committee on Social Policy and Administration, 1989: 7).

But not everyone was happy with this image of being a theoretical subject that sociology was building for itself during the expansion. A number of people saw it as unhealthy and unworthy; as 'full of vacuous theoretical jargon which did not tell [them] anything about anything at all' (Author's Interview with Lindsay Paterson, 2017). Writing in 1968, Andreski admitted that, unfortunately, among sociologists there was a common 'addiction to nebulous verbosity'; that 'under the cover of pretentious jargon we are served platitudes, trivialities and inferior versions of very old ideas' (Andreski, 1968: 397). Others criticised the exclusive focus on theory, arguing that British sociologists' attempts at theorising were falling short and were limited to 'idea-shuffling' while lacking in their ability to make a contribution 'to the mainstream theoretical debate' (Jackson, 1975: 27). In its attempt to theorise about the social world, sociology was offering 'really uninteresting and badly written' prose to those who could turn to the British empirical tradition of social research when they were looking for 'precise thought which would be able to be generalised'; or who could turn to the great novelists, such as Dickens, Flaubert or Tolstoy if they 'wanted to find out, let's say, about the lived reality of the social conditions let's say of nineteenth century Europe' (Author's Interview with Lindsay Paterson, 2017). For

these people, sociology was striving towards a middle ground – between literature and science – that actually *was not there;* at least not yet.

Non-quantitative: Sociologists' Views on Statistics and Quantitative Methods During the Expansion

The vagueness of sociology, which facilitated its expansion; the uncoordinated manner in which the subject was expanding; the ideas with which sociology was associated – innovation and radicalism; political engagement; and preoccupation with theory – all played a role in creating more difficult conditions for the development of 'statistical sociology' or even a statistical side of sociology. But in order to explain more fully why the position of statistics in British sociology during the expansion became as marginal as it did, we need to know how sociologists thought of statistics at the time and how they understood its role in sociology. What was it about statistics that sociologists commonly believed, in addition to their related beliefs about the general nature of sociology, that made it difficult to accept statistics within sociology?

The period of the academic expansion of sociology was rich in discussions about which methods are suitable for sociology and which are not. But unlike the discussions that took place in the 40s and 50s, which rarely focused on particular methods and followed more general terms such as quantification versus more qualitative approaches, the discussions during the 60s were more specific, singling out particular methods, most notably criticising the survey method, in an attempt to prescribe an appropriate methodological strategy for sociology (usually based on non-quantitative methods). The debates surrounding positivism in British sociology that took place during the late 70s and early 80s had their precursor in these earlier discussions and bear similar characteristics.

One of the most striking things about discussions of the methodological make-up of sociology in the 60s is that there is very little reference to statistical methods *per se* or the issues of training in statistics. Instead, all discussions that talked about

'quantification' in any sense, focussed on the survey method, something which persists to this day:

> I would speculate that people still associate quantitative methods with surveys […] I think people are not aware as they should be of the potential of those kind of imaginative and in a sense non-survey uses of statistics (Author's Interview with Donald MacKenzie, 2017).

Writing in 1961, Edward Shils, an American sociologist working in Britain, denied that academic sociology could benefit from associating itself with the empirical and specifically quantitative tradition of social research that had earlier developed in Britain. He argued that the tradition of political arithmetic 'contains neither problems nor themes of investigation' while the social survey 'carries with it an almost inevitable superficiality and flatness' (Shils, 1961: 5). The tradition of political arithmetic was further understood to be 'partly responsible for the dryness of much of British sociological research today. Neither political arithmetic nor the survey fosters the study of actual working of institutions' (Shils, 1961: 5).

Thus from the early post-war period the survey method was not perceived as part and parcel of 'scientific sociology', even though at that time the idea of what comprises 'scientific sociology' was still unclear; surveys were more likely to be understood as an example of 'scientism'[27] rather than science. 'Ours is an age of scientism', argued MacRae, and 'some of this scientism is involved with numbers, particularly statistics' (MacRae, 1969: 601). And again, despite acknowledging that sociology cannot completely do away with quantitative work, a common conviction among burgeoning sociologists was that 'to count is to reduce to units' which either 'does violence to the richness of the data' or 'involves an ultimately destructive process of erosion to the validity of the statistical data'; and that statistics can only prove the existence of already known facts (MacRae, 1969: 601). Sociologists emphasised this distinction every time an external authority, such as the SSRC, attempted to influence sociological thinking and research by recommending more

[27] Scientism is an excessive, often unjustified, belief in the power of science.

and better quantitative training[28]. At the time when the Heyworth committee was working on its report, Marshall, for instance, argued that the views of those in charge of that committee did not represent 'scientific sociology' but 'rather social studies using scientific techniques to collect and present factual information' (Marshall, 1963: 26).

These examples suggest that British sociologists were unwilling to engage with statistics for reasons that were epistemologically grounded; that they saw little value in statistical or survey work; and that they believed in the importance of *resisting, rather than mastering*, the growing power that statistical analysis was exerting in many fields of life. Not everyone, of course, shared these views – some, like Peter Townsend, argued that the future of sociology may depend 'more than anything else, on the question whether statistical survey techniques can be married to the complex but often inexplicit techniques of personal observation and description' (Townsend quoted in Marshall, 1963: 26); and Peter Abell was adamant that until British sociology realised 'the symbiotic relationship between theoretical elaboration and research technique' it would 'continue in the type of sterile theoretical debate that is so characteristic of much sociology (Abell, 1966: 625). There is little sign, however, that the course of post-war sociology in Britain was influenced by views, such as Abell's or Townsend's. On the contrary, 'the dominant people in sociology had no interest in statistics, so it was rather like hiring a technician' (Author's Interview with John Wakeford, 2017).

And, indeed, to many sociologists during the expansion, survey research was no more than mere technical work, to be done by 'technicians'. Moreover, survey work was being described as a technical menace that was threatening 'to take over the discipline, leaving little scope for a discussion of its fundamental theoretical problems' (Rex, 1966a: 529). According to Rex, there was some hope that the 'simple technicians' who were merely engaged in processing interview schedules on a computer might retaliate and that sociologists like C. Wright Mills, Peter Berger and Aaron Cicourel, who were said to have effectively exposed 'the inadequacies of the sacred methods of survey research' will take charge (Rex, 1966a: 529). Such

[28] For a detailed discussion of the disparity between the views of statistics and recommendations contained in official reports and the views of majority of British sociologists on these topics, see next chapter.

views were not peculiar to the period of the expansion but continued into the 70s – Roberts who entered sociology in the 70s still remembers that among sociologists there was

> a slightly arrogant dismissal of [statisticians'] skills in the same way in which in the nineteenth century the middle classes treated doctors…as if they were sort of a slightly higher class of servants (Author's Interview with Helen Roberts, 2017).

Attempts to make 'real' sociological contribution by using the survey method were not denied or actively sabotaged; Rex, for instance, points out that there are areas of sociology where this could be done. However, such work was not encouraged and was being left, more or less, to chance – 'By all means let mathematical sociologists make their contribution where they can' (Rex 1966b: 661), leaving the impression that any such contribution would be of little relevance and importance to sociology which needed, first of all, 'a sociological theory far less insular in outlook' (Rex, 1966a: 529; Rex 1966b: 661). Even though there was little active suppression of survey work, attitudes towards surveys, defining it as scientistic, merely technical and arid, help to explain the root of the problem with teaching quantitative and survey methods to students in this period.

Surveys were also seen as distorting the image of sociology in the public perception. During the 1960s, the fear spread that, lest sociologists take some action, their subject would become unduly associated with survey work – with 'what' and 'how many' questions, but not sociological 'why' questions. In a reflection on the development of survey research, Mark Abrams pointed out that:

> In short, never before has so much survey research been carried out in Britain in the social sciences and never before has its discipline-scope been so wide – from geography to criminology, from political science to sociology, from education to business management; and never before has there been so much survey research aimed at policymakers (Abrams, 1974: 3).

It could be argued that the abundance of social survey research conducted in the commercial, governmental and local authority sectors, encouraged sociologists to believe that they should differentiate their work more clearly and decisively and

claim authority over the use of different approaches, for instance, theoretical approaches. The anxiety was that that unless sociologists assist them, the public would not be able to make the distinction between sociology and mere fact-collecting and would identify the *whole* subject of sociology with 'the *technique* of survey research' (Bechhofer, 1967: 838). Some damage, it was feared, had already been done: 'the sociologist has become identified in many people's minds as a man who carries out surveys to obtain the factual information about people which everyone knew in advance anyway' (Bechhofer, 1967: 838). It was high time that sociologists reverse that trend; making it 'common knowledge that sociology is concerned with the structure of society and the working of social systems, rather than with the obtaining of facts about people as an end in itself' (Bechhofer, 1967: 838).

Contributing to the 'bad image' of surveys among sociologists was the tendency to quote examples of bad quality survey research (for insightful examples, see Petersen, 1966: 11) and to consistently ignore the fact that there was also good quality survey work being done. Although it is important to be wary of any possibilities that methods are being misused, it is worth noting that, to my knowledge, there are no existing record of discussions or commentaries on any other methods apart from the survey method that outline in such great detail, *through bad examples,* the potential harm that misuse can cause. The point is, therefore, not that sociologists were wary, but that they were disproportionately more suspicious of quantitative methods, especially surveys, than other methods. This strategy had a pervasive impact on the general opinion about such work among sociologists and was openly criticised by statisticians:

> While anti-quantitative social scientists undoubtedly can find ammunition for their views, to observe and concentrate on a few cases of abuse is hardly good evidence in general. Anti-quantitative attitudes can lead to situations where large amounts of data remain unanalysed, when even simple analyses would provide insight into the subject matter. […] I once had the salutary experience of being criticized for using a decision theory model on the grounds that similar models had been used by the US military in Vietnam! (Goldstein, 1984: 264).

In a more recent interview, Goldstein reaffirmed these observations (Author's Interview with Harvey Goldstein, 2017). Wakeford, who had been active in

sociology in the 1970s, still argues that 'the quality of what goes in statistics is usually extremely suspect' (Author's Interview with John Wakeford, 2017); the quality of what goes into qualitative methods, however, does not get questioned.

Where did the idea come from that survey work is scientistic, dangerous and misleading? Where did the idea come from that futile and blind survey research was in danger of taking over British sociology? These ideas could not have come from the earlier British experience with sociology for two very good reasons. Firstly, British sociology's development as an academic subject was stagnant up to the point of the expansion when these views on surveys became especially prominent. In addition, British sociology had developed and continued to develop in parallel to, but separately from, the tradition of social empirical research practised in other university departments. Therefore, there is little to suggest that the views on surveys discussed above were established on the basis of British sociology's *own* experience with surveys or with quantitative methods more generally; there is also little to suggests that sociologists were competent enough to criticise surveys, given their lack of experience in this kind of research. Material from recent interviews with sociologists, who were active at the time, confirm that British sociology's experience with quantitative methods was barely sufficient for reaching such uncompromising conclusions: Paterson reports that quantitative social research was 'almost like a ghetto […] of rather arcane activity, completely separate from the mainstream sociology' and that if 'we look at the thing called sociology, then I doubt that there was ever an overwhelmingly quantitative past' (Author's Interview with Lindsay Paterson, 2017); while Bechhofer is clear that:

> If someone said to me that sociology moved […] out of a quantitative past, my response would be what quantitative past? Show me! Because how many quantitative studies can you name from that period (Author's Interview with Frank Bechhofer, 2017).

If not from sociologists' own experience with quantitative and survey methods, then, where did this hostile attitude come from? A major factor in shaping the attitude of British sociologists towards quantitative work was their perception of the state of American sociology and the role of quantitative and survey work in American sociology. In fact, most of the evidence of bad quality survey research and

potentially harmful survey methods cited by British sociologists relates to the American experience and comes from American sources. For instance, in discussing surveys, Donald MacRae, who in 1963 was the editor of the *British Journal of Sociology*, commented that it was

> one of the most frequent errors of sociologists in America to force their students into what is believed to be the mould of say, physics, and assume that the banal, trivial, or irrelevant thus takes on worth and significance. It is despite of this that American sociology now leads the world (MacRae, 1963: 6).

What we see in MacRae's comments is not only a rushed generalisation that all quantitative or survey work is a failed attempt to merely imitate 'natural science' but also a conviction that such attempts can only hamper the progress of the discipline – if America can succeed in spite of this, MacRae hints, this might not be so easy for British sociology which had not yet established itself firmly enough. The denunciation of quantitative and survey work was here justified as being a way to avoid the 'mistakes' that sociologists were making in the USA. It was on the basis of the perception that there was too much survey work being done in the USA and that too much of it was of poor quality that British sociology could claim, regardless of the lack of first-hand experience, that, there was overall 'too much faith in the quantitative, and, in the quantitative, too blind a belief in the identity of science with the metrics of probability' (MacRae, 1963: 8).

Such views were fuelled by UK publications of American critiques of survey research. Writing in *New Society*, William Petersen from the University of California, Berkley, argued that 'some survey research is ethically dubious' and that 'some is expensive nonsense'; that a great part consists of 'reported opinion and reported attitudes'; that data are collected mostly by 'amateurs'; that survey research not only often 'distorts reality' but that it 'itself creates the facts it analyses' leaving behind an 'indigestible mass of unrelated facts' (Petersen, 1966: 11). However, even if there were research in the USA matching the characteristics that Petersen outlined, it is only realistic to believe that there was a lot of 'good quality' research being done alongside it; the records of *New Society*, however, show that they did not publish articles showing this. Petersen's critique, and other similar critiques, might contain

some truth, but could also be dangerously misleading for British sociologists when taken on their own. Margaret Stacey, for instance, fully agreed with Petersen, replying that 'we should take this timely warning from the United States before we are overdone with surveys' (Stacey, 1966: 31). And this is still the belief nowadays:

> I think it was the…almost the domination of quantitative research in the American journals that really put people in Britain off quantitative methods […] one of the elements, I think, in British resistance to quantitative methods was a feeling that there was this awful tradition in American sociology which was very quantitative and functionalist at its best and trivial at its worst (Author's Interview with Robert Moore, 2017).

> America, of course, became more and more and more and sometimes stupidly quantitative. There were years when the *American Journal of Sociology* and the *American Sociological Review* were well-nigh unreadable because it's just so boring sociology (Author's Interview with Frank Bechhofer, 2017).

Whether American survey work was of the predominantly 'unreadable' and 'boring' and un-insightful type is not for us to judge here. Suffice it to say, however, that impressions of the existence of such work in America, in whatever proportion, helped fuel an overall negative attitude towards survey work as a whole in Britain; and that British publications, like *New Society*, and sociologists discussing surveys were receptive to the bad survey press, using it to justify sociological work based on other approaches that did not contain quantitative data collection or analysis. It was as a result of the *receptiveness* towards the bad survey press coming from America that British sociology *could adopt a negative attitude about statistics without having to first experience statistics itself*. In the 1960s, surveys were the new 'Drink, Drainage and Divorce' (which was used as a derogatory term for empirical social research in the early twentieth century).

If these were the attitudes of British sociologists towards survey work during the period of the expansion, what about their quantitative skills? It is hard to say anything conclusive and precise about the actual abilities of British sociologists in doing statistical and survey research due to lack of primary evidence. The evidence that is available, however, suggests that lack of ability was not *the main* reason why the incorporation of these methods into British sociology remained a rather remote

possibility; often, lack of ability was normalised and legitimised using such views, as outlined above:

> for a long time there has been this…dangerous conflation of two separate things: first of all, that to do statistical work you do need a reasonable mathematical background which many people studying sociology and indeed teaching sociology don't actually have; together with [...] the suspicion that there is something conservative about quantitative methods; of course, the second of those can act as a source of legitimation for the first of them (Author's Interview with Donald MacKenzie, 2017).

One of the most common arguments about the lack of statistical abilities in sociologists and sociology students in Britain is that this results from mathematics anxiety and lack of basic mathematical skills. However, what is more important than the existence of such anxiety, assuming such anxiety did indeed exist, is that in the crucial period of the expansion, when a whole new generation of sociologists was being educated, there were plenty of opportunities to legitimise lack of quantitative skills on the basis of epistemological and political views, such as views that quantitative and survey methods are conservative or positivist.

Non-quantitative: How did Student Characteristics and Expectations Affect the Incorporation of Statistics in Sociology during the Expansion?

As I mentioned previously, the democratisation of education and the changes that went with it created conditions that were particularly favourable to the expansion of the social sciences; specifically, sociology. An important change, as far as student demographics was concerned, was that as a result of the expansion of education a lot more women were entering higher education and there is evidence to suggest that a disproportionate number of women entered university to study sociology, compared to other subjects or subject groups. Evidence for this is presented in Appendix I in Tables 13 and 14 and in Appendix II in Figure 2, showing that consistently throughout the 60s and 70s, the women/men ratio in sociology was more similar to that in the art subjects than in science subjects or economics; and that the proportion of female students in sociology was consistently the highest. So another important

factor that stimulated the expansion of sociology was that there was something in the image of sociology that made it more attractive to girls; and something about girls that made them more likely to be attracted to sociology. What was it?

A partial answer could be derived from the ideas with which sociology was associated. It is well known that women are more likely to be attracted to the arts than the sciences; the fact that, as I explain below, sociology promoted itself consistently as a general education subject, with similar objectives to arts subjects, is likely to have played a role in attracting young women. The theoretical, radical, holistic and innovative subject characteristics that sociology boasted, gave sociology a general appeal across both genders. But for girls, many of whom were from families where young women did not study for a higher degree, sociology held out a more special promise:

> sociology has become for girls […] what English literature was for the last generation at university – safe because everyone you know is doing it and no one can be actually 'bad at it' (*Unknown Author*, 1970: 17).

Adding to this, there was a widespread belief that a degree in sociology, unlike a degree in English literature, would help women 'do good': throughout the 60s and the 70s, women choose to study sociology with some thought of becoming social workers (Smith, 1982: 152; Stewart, 1989: 151). But there is also some evidence suggesting that another important factor in young women's choice to study sociology was the *lack of requirement* to possess quantitative skills or engage in quantitative data collection or analysis. The evidence below does not specifically refer to women, rather it talks about sociology students as a whole; but, knowing that the majority of sociology students has consistently comprised of women, it could at least be hypothesised that students' gender influenced the expressed opinions; and that the fact that sociology in Britain has consistently been a 'women's subject' could also partially explain why, in the absence of stronger factors influencing the situation in the other direction, sociology in Britain has also tended to be non-quantitative.

The first piece of evidence that points in this direction comes from a survey of undergraduate sociology students conducted at the LSE in 1968. Two thirds of all sociology undergraduate students at the LSE filled in the survey. Of these, only one

third considered statistics as an essential sociology subject (LSE, 1968). Two years later an investigation by *The Times* reported that 'many [sociology] undergraduates are horrified to be confronted by algebra, computer techniques and the often (pseudo) scientific jargon of the sociologist'; the view common among sociologists that 'methodology has become an obsession among sociologists today and for many it has become an end in itself' have certainly had an effect among the students too (Unknown Author, 1970: 17). A report by the Interdepartmental Working Group on Computer Education confirmed these tendencies, specifically among social science and arts students:

> For these students, motivation is particularly important. Many of them have thankfully given up mathematics, and the supposedly entirely formal thinking that goes with it, at least two years before entering university. To show them how a computer can be made to do things which they do not see to be valuable is of little use. It is also unwise to assume that the mathematical competence of these students remains at the level reached when they ceased to study the subject; a great deal would have been forgotten. […] *But the main difficulty remains as lack of motivation*. Most of these students intend to specialise in economics, government or sociology. In their first year they do not see the reason for studying statistics or computing, and *many of them would prefer not to know about these topics*! (Computer Board for Universities and Research Councils, 1970: 17-18).

During the expansion period, sociology, more than any other subject, relied for its academic existence on student demand and the preferences of students, mostly the female students, who came to sociology; making quantitative modules *compulsory* would have had a negative effect. Donald MacKenzie, for instance, described how after he taught statistics as a compulsory subject, in the 70s, he had little choice but to make the statistics course optional, because he felt that 'you're better off having a class where the people doing it really want to do it rather than are being forced reluctantly to do it' and that the compulsory statistics course was 'dragging down second year numbers' making him think that the department was 'killing sociology by putting something into second year that students don't want and don't see the need of' (Author's Interview with Donald MacKenzie, 2017). Another example comes from Robert Moore, also a sociology teacher in late 60s/early 70s:

> I suspect that quite a few students come into sociology because they don't want to do any subject that involves numbers…they don't want to do science, they don't want to do maths, they are really in flight from numbers because they want to talk about society and human activity and some of them wanted to talk about theory, they wanted to understand how the world worked but they didn't want to do numbers. So we had always a significant number of what you might call number-averse students. [...] I can't remember when there weren't number-averse students (Author's Interview with Robert Moore, 2017).

What is noticeable, although highly speculative and hard to prove, is the mutual legitimation of, on the one hand, the views of the first- and second-generation professional sociologists who in the majority were either dismissive or hostile to quantification of any sort, and, on the other hand, the understanding of the students about their own abilities and their expectations of sociology:

> The view was, well, if students really wanted to do that kind of thing, they can, but it doesn't form an essential part of sociology, why should they have to do this sort of thing? And that of course was coming from quite a few members of staff, who themselves couldn't do that kind of work and you could see them switch off if you talked about a confidence interval (Author's Interview with Robert Moore, 2017).

There have even been even cases in which students have outwardly rejected sociology teachers' attempt to teach them anything quantitative. Platt reported that when she was a young sociology teacher in the 60s, a student came to her after a methods teaching section in which she discussed surveys, and told her:

> 'Of course, what you were doing was just trying to make us be sort of hapless tools in hands of the state, weren't you?'…Every seminar was 'A critical approach to…' [but] it was this violent ideology that was applied irrespective of whether it actually fitted the circumstances which was a bit hard to deal with (Author's Interview with Jennifer Platt, 2017).

It is possible that these views towards quantification were widely observed because sociology was primarily a 'women's subject' – at least as far as undergraduate students were concerned. But in the absence of data for the period under investigation, this remains a speculation. What we do know, however, shows clearly that as the expansion of sociology was taking place, the self-image that sociology was in the process of creating and female students' expectations of what

sociology had to offer, together made it more difficult to introduce statistical training into sociology. 'The consumer was king' (to use Mandler's 2017 phrase) not only in the higher education system more generally during the expansion, but also in sociology, especially when it came to its methods choices.

Beyond the Expansion

This chapter examined the expansion of sociology in the post-war period from both the perspective of the expansion of the education system and the perspective of the historical development of sociology prior to 1945 and showed what the effects of the expansion were on the possibilities for achieving some degree of incorporation of statistics into sociology. It also attempted to modify and improve our understanding of the expansion itself through the lens of the historical relationship between statistics and sociology. We have learned new things about sociology's expansion by looking at its relationship with statistics; and new things about sociology's relationship with statistics by looking at its expansion.

Unlike existing accounts which concentrate on the expansion primarily as an opportunity, the present analysis shows that this period also posed unexpected and unusual challenges for a subject that was underdeveloped and undecided about its subject matter and methodology. This is clearly seen in comments that some contemporary sociologists made while reflecting on the expansion. As early as 1963, Little argued that the 'eclectism' that was emerging as a result of the uncoordinated expansion was a big 'disadvantage' that was responsible for the amateurish nature of much of social and sociological research'; 'amateurism in design, methods and conceptualisation' was rife (Little, 1963: 69). The variety of ideas, approaches and methods that mushroomed as quickly as the number of professional sociologists resulted in many researchers lacking even 'basic training in the social sciences'; and a sociological curriculum which by 'emphasising the diverse traditions and strands among British sociologists' was producing graduates who had 'contact with a wealth of approaches to the study of society but insufficient expertise to utilise any of them (Little, 1963: 69).

In retrospect, it was also reported that unconditional expansion allowed any previous commitments to empirical research to falter, especially under extreme forms of both ethnomethodology and neo-Marxism (Westergaard and Pahl, 1989: 379). One argument was:

> With the large increase in faculty members, many sociology groups were able to become rather more autonomous […] As a result, the position of SST [Social Statistics Teaching] within the sociology curriculum gradually came under scrutiny in many departments […] An eclectic borrowing of both personnel and theory was therefore occasioned by the postwar growth of academic sociology […] (Irvin and Miles, 1979: 20-21).

By 1970, even in the eyes of the public, sociology was seen to be 'riven with discontent', with departments reported to have been failing to 'agree on what constitutes sociology' (Unknown Author, 1970: 17). And this was expected to have negative consequences for one of British sociology's most valued assets: incoming student rates. As I explained earlier in the chapter, the majority of students had chosen to study sociology in the belief and with the hope that sociology would fulfil their aspiration to change the world for the better; as early as 1970, it was becoming clear that it was very likely that sociology had raised student expectations so high, that it could not possibly fulfil them:

> Unreasonable hopes for sociology make disillusion with it all the more profound. Just another God has failed, said a gloomy dropout. But it's not only dropouts that expect too much of sociology. There is disillusion among staff as well as students (Unknown Author, 1970: 17).

In such precarious circumstances, sustaining the subject's growth was becoming more difficult: because 'sociology raised student expectations in the early 1960s beyond those it either could or should try to meet' by the early 70s sociology faced the risk of becoming 'imprisoned in its sudden popularity' (Westergaard and Pahl, 1989: 379).

Whether such a strong degree of fatalism was justified or not is a matter for another enquiry. What we do know, is that the expansion of sociology could not have happened without external educational reforms, which in turn played a fundamental role in determining what subject sociology could or could not be. Sociology had the

freedom to associate itself with almost anything that was new, radical, female-friendly and politically progressive; anything that inspired faith in sociology as an academic subject. Because it was a subject which relied heavily for its existence on student demand, sociology could hardly afford, even if such attitudes were present, to establish statistical competence as an essential, an indispensable requirement. Positive, embracing and encouraging attitudes to any sort of quantification, including survey work, were absent in this period; moreover, it was primarily attitudes, not ability or lack of ability, that were the main reason why the incorporation of quantitative methods and thinking into sociology remained only a remote possibility. It was the *way* British sociology expanded, not the *fact* that it expanded, that had the major influence on the subject's relationship with statistical and, more generally, with quantitative research.

The 60s expansion of sociology was a phenomenal event, but it was neither unique nor unusual, at least as far as the history of social science in Britain is concerned. The 1830s and 1840s, where the history of social science proper begins, was a time of similarly radical and rapid social change; a time when the very idea of social science emerged and was for the first time discussed and practised not just by philosophers and reformers, but also by statisticians, with the hope that it will provide the source of Enlightenment needed to grasp all social change and to bring about outcomes which were socially desirable. The emergence of social science in this period was characterised by a belief in social science before social science had proved it was capable of bringing about social Enlightenment – a belief which is very similar to the 60s belief in sociology, which rose regardless of the fact that sociology had little to offer on a practical level. So the 60s expansion of sociology has a clearly recognisable precursor in the 1830s/40s emergence of social science. In both cases, however, statistical techniques and the statistical way of thinking about society were in the periphery of the strictly sociological project. Numerical facts alone do not emit light, ideas are necessary – and sociologists, in particular, both in the early nineteenth century and the early post-war period were looking for light, for hope, for new beliefs and ideas, not for new *facts. And facts were all they could see in statistics*.

Chapter Twelve

The Teaching of Sociology to Undergraduate Students in Britain, 1904-1979

This chapter examines in greater detail the development of the academic characteristics of sociology as taught to undergraduate students throughout the twentieth century. Unlike previous chapters which were limited to a particular time period, this chapter requires a different approach – as changes in teaching tend to occur slowly over time, pervasive trends can only be observed over longer periods. The analysis of the teaching of sociology to undergraduate students in this chapter therefore spans from 1904 to 1979.

The chapter begins with an analysis of the understanding of the nature of sociological education as sociology developed from a relatively obscure academic subject in the interwar period to one of the most popular academic subjects. Was sociology going to be more like an 'arts' subject, providing a general education; or was it going to be more like a 'science', focusing on methodological instruction and, where suitable, vocational opportunities? Discussion in previous chapters has already suggested that the first option was the more likely of the two but this chapter provides further evidence to reveal how this came to be the case.

After establishing what the overall academic characteristics of sociology were throughout the twentieth century, the chapter continues with an examination of how sociology curricula were structured and what they contained; what the place and role of methods courses in curricula was; and what proportion of the curriculum was devoted to statistical and survey methods. The analysis is based on evidence from the LSE curricula, comparing sociology, social science and administration and statistics courses; and from the collections of sociology methods courses syllabuses compiled by Peel (1968) and Wakeford (1979) and from material from the student guide *Which University*/ *Which Degree* (1969-79). The amount of material available for this type of analysis is limited in its scope; but combining information from the LSE calendars and the syllabus collections should allow us to establish with reasonable certainty what sociology teaching comprised of during and after the expansion and

how it compared to other relevant subjects. Many aspects of the teaching of sociology at undergraduate level changed over the twentieth century, but as this chapter shows, there was also a remarkable amount of continuity.

It should be made clear that instead of focusing on the types of research or research fields or methodological approaches to research that are most common (cf. Madge, 1957; Carter, 1968; Collison and Weber, 1971); or on the development of whole degrees (Fincham, 1975); or topics that were prominent in the curriculum, this chapter focuses on sociology as a *set of skills* and asks what skills sociology students were required to learn as part of their sociology degree; what they were expected to be able to *do* in order to become qualified sociologists. Although the amount of attention this issue has been receiving lately might give the impression that this is a more recent concern (Williams et al, 2004; Williams et al 2008; MacInnes, 2010; Platt, 2012), this chapter shows that it is an issue with a history, a history as long as the history of British sociology itself. Evidence that this has indeed been the case emerges from an analysis of the discrepancy between the recommendations of official reports with regards to quantitative teaching and sociologists' views on this issue; and also from an analysis of the American view on sociology teaching in Britain, both of which are discussed towards the end of the chapter.

Perceiving Sociology as General Education

The analysis of the views expressed by sociologists in Britain during the first half of the twentieth century and in the early post-war period in previous chapters has already provided a clear indication that, throughout this time, British sociology has been continuously, and almost without any exception, oriented towards a liberal education aimed at broadening the mind with philosophical investigation and theory building. Particularly in the early post-war period, we have seen that there was a clearly pronounced methodology outlook among sociologists that assumed that methods training, or any potential vocational elements that sociology may have, were not viewed as essential and, if necessary at all, could be provided at the postgraduate level. But could it be the case that there was a more widely pervasive trend that

sociology in Britain was continuously understood as a subject aimed at providing a general education? If this was indeed the case, then this could help to further explain how the conditions in Britain throughout most of the twentieth century made it difficult to establish a close and secure relationship with statistical methods – such methods would have been, at best, seen as a topic sociology students could learn about; and, at worst, seen as incompatible with the understanding that sociology should provide a general, liberal education.

There are numerous historical documents produced between 1914 and 1983 that help us understand more clearly what sociologists thought sociology should offer to students, with the majority of evidence suggesting that sociology in this country was developing as a general or liberal education subject.

The 1914/5 LSE calendar, for instance, contains a description of some sociological courses taken by the students studying Social Science and Administration (SSaA). The role of the sociological courses in the SSaA degree was:

> not to teach them to theorise but to deepen their intelligent interest in everything connected with their subsequent practical work. It is necessary that students desiring to take the full course should possess a good general education (LSE, 1914: 92-3).

This is a clear statement of how sociology was seen by social science colleagues working in the SSaA. Similar descriptions continued to appear in the LSE calendars throughout the twentieth century, supporting the idea that their understanding of sociology was as a useful addition providing a general education element to more vocationally oriented subjects.

The first time the topic of the teaching of sociology was formally debated was at a 1936 symposium called *The Social Sciences – Their Relations in Theory and Teaching*. At this symposium, it was argued that although certain political and cultural changes affecting the health and well-being of large populations can be measured using statistics, this does not go very far in enabling sociologists 'to solve the weighty and more general problems of the art of government' (Hobson, 1936: 19). Furthermore, in discussing the place of sociology, Morris Ginsberg and Karl Mannheim confirmed, yet again, that sociology is a *general* social science. Even social scientists like Alexander Carr-Saunders who, by virtue of their own work,

might have been expected to conceive of statistical investigation as an essential part of sociology, described it as vital but, nonetheless, *external* to sociology: 'Statistical sociology is for the sociologist what archivalism and archaeology are for the historian' (Carr-Saunders, 1936: 211). But Marshall, who reported on the provision of sociology courses, was less conclusive: 'the present position is chaotic' and 'we have not really made up our minds what it is that we are trying to do' (Marshall, 1936: 55). In the light of such comments, one cannot not help but wonder whether previous and later attempts to define sociology as a 'general' or 'liberal' education were not, at least partly, attempts to disguise the chaos, confusion and ambiguity that dominated sociological thinking.

Thus, when the expansion gained momentum, instilling unprecedented optimism into the sociological community, talk about the 'chaotic' position of sociology ceased but talk about sociology-as-general-education did not. The 1960 *Guide for Intending Students* prepared by Asher Tropp and Joe Banks for the BSA asserted that sociology provided 'sound liberal education' (Tropp and Banks, 1960: 11). In 1963, Little observed that sociology was 'certainly' the 'type of general education that many of the supporters of new universities […] hope to see offered' (Little, 1963: 70). This suggests that the idea of sociology as general education was seen as *instrumental in sociology's expansion*, as facilitating that expansion; providing undergraduate students with:

> …precision of thought and reasoning, an acquaintance with moral and value problems, a historical perspective and detailed knowledge of the variety, complexity and problems of contemporary societies (Little, 1963: 70).

In addition to helping sociology expand in universities, sociology-as-general-education would help the subject secure a place at other, more vocationally oriented, higher education institutions, such as Colleges of Advanced Technology and polytechnics, since:

> …students at these institutions must spend part of their time in general or liberal studies and already various aspects of sociology are being used in this attempt to give general education to scientists (Little, 1963: 67).

It is of course impossible to say with any certainty that perceiving sociology as a general education subject *led* to an unappreciative attitude towards methodology, in general, and quantitative methods, in particular, but such a perception would inevitably create an atmosphere conducive to such attitudes. For instance, a survey of sociology undergraduate students done at the LSE in 1968 shows that, when asked about five subjects that should form the core of the sociology syllabus, 80% of respondents answered 'theory', 69% 'philosophy', 56% 'social psychology' and only 30% 'statistics' (LSE, 1968). Given that the response rate of the survey was 58%, we can conclude that of the nearly two thirds of undergraduate sociology students who filled in the survey, only one third considered statistics an essential sociology subject. The view that statistics is not essential to sociology is also confirmed by the answers given by the students who studied sociology as a special subject as part of *another* degree (BSc Economics with Sociology & BSc Economics Part 1; and BSc Geography students taking 'Elements of Social Structure'). *An even smaller* proportion of these students reported that statistics was a core element in sociology – 14% and 12% respectively. Part of the explanation could be that these students received statistical training as part of their economics training, or even separately; but these results also show that students, coming to sociology from different subjects, also shared the notion that sociology is not a subject with a strong statistical element.

Less than a decade later, Clarke (1976), conducted a survey with first-year sociology teachers on the conceptions that first-year sociology courses were attempting to convey. He was interested in finding out whether there was a consensus on these conceptions and whether first-year courses attempted to portray sociology as a 'liberal humanistic subject' or 'as a science' or 'as a profession'. He found that, according to sociology teachers, the main difficulty in teaching sociology to first-year students, was trying to put across 'complex issues and sophisticated ideas', 'the personal and non-technical nature of the subject' and the necessity for 'thinking critically about social reality' (Clarke, 1976: 89). Clarke's results show clearly that, for the teachers, sociology was far from a technical subject; teaching sociology with the aim of providing a general education need not involve statistical training or survey techniques.

Further evidence comes from teachers' answers to the question 'How would you summarise the conceptions of sociology the course tries to convey?' (Clarke, 1976: 91). The majority of answers pointed to: 'the sociological perspective', 'thinking sociologically', 'think critically', 'recognise the givens in society as problematic and sociology as worthy of critical imagination'. There are some answers suggesting a desire to claim empirical relevance for sociology; but nothing in the responses suggests that sociologists were aiming at preparing their students for the idea that sociological research is *more* than thinking critically nor is there any mention about acquiring the ability to conduct empirical research.

Similar attitudes are to be found at postgraduate level. A 1971 survey by Mark Abrams shows that ca. 60% of social science postgraduates who had received SSRC grants, regarded 'any statistical training' as 'relevant' (Abrams, 1974: 4). Although the proportion of students who thought statistical training relevant is bigger than those did not, the results are ambiguous because they do not specify the rate for sociology students in particular and also what kind or level of statistical training students thought was essential. The survey, however, also revealed that the majority (ca. 60%) of those who thought statistical training was relevant described it as 'either mediocre or poor'; only 7% described it as excellent (Abrams, 1974: 4). It is difficult to say whether sociologists' views of statistics influenced the poor quality of statistical training offered to students or vice versa; regardless of the direction of influence, the end result for a couple of generations studying during the post-war period was a poor ability to do quantitative research and a lack of enthusiasm for engaging with it at all. Later on, in the same report, Abrams showed that only 14% of the PhD theses written by the interviewed social science postgraduate students could be classified as 'certainly quantitative' (Abrams, 1974: 4); with another 16% as 'possibly quantitative'. A similar exercise was done in 1974, using a BSA register of PhD Theses in Sociology from that year (ca. 1000 theses). Results were very similar, showing that the percentage of quantitative work across social science subjects was very similar to the percentage of quantitative work *within sociology alone* – 8%

could be classified as 'probably quantitative', 26% as 'possibly quantitative' and 66% as 'almost certainly not quantitative'[29].

Abrams' report also revealed that both students and staff regarded social science as closer to the liberal side of education than to the scientific and the vocational and that among sociology staff there was an outright hostility towards any attempts to change that. Abrams remarked on the 'contempt with which some senior social scientists regard the skills of numeracy'; he also recalled that when the SSRC advertised financing for research similar to the Detroit Area Study, 75% of British universities showed no interest. When Abrams visited some of them, he was met with comments amounting to "There will be no survey research in this department except over my dead body" (Abrams, 1974: 4). This experience resonated well with Jackson's (1975) more general view that:

> British sociologists are rarely very numerate individuals. In spite of the widespread use of computing facilities […] there is a remarkable lack of interest and training in quantitative measures among many postgraduates (Jackson, 1975: 26).

Statements like these, however, do not appear to have raised any alarms among leading sociologists. The 1986 BSA presidential address re-affirmed the value of a sociological general education arguing that while 'a few people' who have studied sociology could 'actually say that in the jobs they do they are 'sociologists", this did not mean that they did not use it in their work (Albrow quoting Urry and Waton, 1986: 339). For Albrow, sociology as 'a more liberal education' taught students how to 'think through problems rigorously, systematically and fully'; it was their ability to 'think in terms of concepts' that helped students fair well in their chosen careers (Albrow quoting Urry and Waton, 1986: 339). Sociology, Albrow was adamant, 'has a leading role to play in asserting the breadth of a humane education' (Albrow, 1986: 345). Little had changed from the early days of the expansion when the idea of sociology as a general education was justified as a way of strengthening the position of sociology and facilitating its expansion; the climate in the 1980s had changed – in Albrow's opinion this was a climate in which 'the narrowest and most vocational

[29] I have examined this register – it gives only titles of PhD theses, which may be misleading. So Abrams' results, although probably pointing in the right direction, should be treated with caution.

possible construction is placed on higher education by government' – but the general education that sociology aimed to provide, his address shows, was perceived to be just as vital. Whether sociology was 'frighteningly fashionable' (Willmott, 1968: 341) or under the threat of Thatcherism, providing general education seemed to remain an unquestioned solution.

Examining this evidence helps explain why it was possible that many sociologists developed a neglectful attitude towards methodology – in a liberal arts degree, methodology would be understood as another aspect to broaden one's mind, not an opportunity to specialise in a particular know-how. This, it could be argued, would reduce the chances that methods teaching reaches its optimum aim – to teach students new skills that they can apply in practice. The perception of sociology as providing a general education can also, albeit partially, explain why empirical skills were not the focus of teaching at the LSE and why the continuous teaching of quantitative and survey methods in the early post-war period failed to resolve the problematic divide that existed between the sociological and empirical social scientific traditions and between sociology and statistics more generally (see below).

The view that sociology is a 'liberal arts' subject has therefore been present in British sociology throughout the twentieth century with remarkable continuity and persistence. These views may have gone unquestioned among the majority of mainstream sociologists but they were met with resistance elsewhere, most notably in the official reports on the state of social science and sociology teaching prepared by government and other official bodies since 1945.

Reports on the Teaching of Sociology and Statistics in the Post-war Period

The understanding that sociology teaching should be organised with the aim of providing a general education was not shared right across the spectrum of social scientists and policy makers involved in higher education. During the post-war period, this view met opposition, consistently and systematically, from a minority of sociologists and intellectuals interested in the teaching of the social sciences in the UK whose voice was made audible through a series of government and official

reports on the topic. A number of these reports show considerable contrast with many of the views coming from sociologists and sociology students discussed earlier. They also show a remarkable degree of similarity and consistency, even repetitiveness, in recommendations. With few minor changes in wording, a report from 1965 could easily be used to describe the situation in the 1980s and even 2000s.

The first major official report that commented on the academic status of sociology as well as the role of statistical training in the social sciences was the Clapham report of 1946. This report was the first major step towards securing more adequate funding for the social sciences: although the committee did not recommend the establishment of a funding body, they were clear that in terms of both staff and research funds, the social science departments in British universities are 'under-staffed and under-endowed' (Para. 3-25). As I mentioned earlier, Clapham expressed certain reservations towards sociology: the report revealed doubts about certain advances in the development of sociology and confusion over its nature. In contrast, when addressing the role of statistics in the social sciences, the Report stated categorically that 'an adequate supply of statistical competence is quite fundamental to the advancement of knowledge of social and economic questions', and went on to suggest that 'there was a chronic struggle […] for the services of a supply of statisticians' (Para. 18).

Another report by Egon Pearson (1947) took the matter further with some clearer suggestions as to how the teaching of statistics could be improved. The conclusions of Pearson's report regarding the importance of statistics as applied to *all* sciences are similar to those in Clapham's report. In Pearson's view:

> […] the statistical approach is so fundamental to the modern way of looking at things – the affairs of everyday life as well as scientific theories and experiments – that it should form part of the mental equipment of the educated man, which it is not at present (Pearson, 1947: 53).

Pearson argued that statistics has been neglected by *all* sciences, while the social sciences in particular appeared to take a rather narrow view of their use, 'almost as another term for quantitative economics' (Pearson, 1947: 52). The report recommended the establishment of elementary statistics courses to familiarise students in the natural and the social sciences with basic statistical ideas and

techniques, as well as more advanced courses 'to enable students to utilise properly statistical methods as developed for application to their chosen discipline' (Pearson, 1947: 54). There is little evidence to suggest that these recommendations were taken up; instead, Clapham and Pearson began a post-war 'tradition' of giving out recommendations which were consistently at odds with what sociology teachers and students wanted and believed in.

Heyworth's report of 1965 did not differ much from Clapham's – sociology appeared to be 'the discipline which people find most puzzling of the major social sciences' (Heyworth, 1965: 3). Heyworth did not show any increase in confidence in, or respectability for, *sociological* research. The report showed only a general increase in enthusiasm for the social sciences overall. But just like his predecessors, who commented on the provision of statistical training, Heyworth was clear that:

> […] Without mathematics and statistics the social sciences as a whole cannot flourish. […] and it is essential that these methods be also developed in universities and colleges so that no student will in the future graduate in the social sciences without a good working knowledge of statistics. […] Our evidence was emphatic that much still remains to be done in respect of the provision, co-ordination and publication of social statistics […] (Heyworth, 1965: 32, 43).

And little had changed by the beginning of the 70s when another report was published, making similar recommendations and remarking upon the failure of universities to endorse such views as were expressed by Clapham, Pearson and Heyworth. Rosenbaum's *Report on the Use of Statistics in the Social Sciences*[30] (1971) aimed to help 'increase the quantity and improve the quality of statistical research within the social sciences (Rosenbaum, 1971: 535). In evaluating the contemporary situation, the Report was clear: the quality of the applications for research grants that came before the SSRC Statistics Committee (est. 1970) was 'poor' and their number – 'disappointingly low' (Rosenbaum, 1971: 535). As regards the quality of the statistical research that was conducted in the social sciences, Rosenbaum found that despite general familiarity with the main statistical techniques, on the whole, 'unsophisticated techniques were used'. Collaboration between social scientists and statisticians was rare and, if it happened at all, it took the form of

[30] Economics was intentionally excluded from the analysis.

consultation and nothing more (Rosenbaum, 1971: 539). Innumeracy among social science students prevailed, a weakness that was thought to have originated at school level. There was a disorganised variety in what was taught and in the manner of teaching; a major problem being how to determine who were best suited to do the teaching – mathematicians or social scientists. In fact, the problem of who would be best to teach statistics in social science departments was one of the most debated, although notably it was always debated by social statisticians (in addition to Rosenbaum (1971), see Conway, 1976; Kalton, 1974; Goldstein, 1984). And although Rosenbaum's is by far the most extensive report on the teaching of statistics in social science from this period, neither the problems he identified, nor the recommendation he made, were novel.

We find further repetition of well-known but unsolved problems in relation to the incorporation of statistics into sociology in the next official report on the state of the social sciences in Britain (ESRC, 1987). The report praised the strengths of British sociology, saying that it is 'generally respected abroad and seen as outstanding in some areas'; but like its predecessors it recommended that substantial progress was still needed in the field of quantitative sociology in which the UK was seen as 'weak' compared to the US (ESRC, 1987: 1). The 'numeracy problem' which British social science was facing was again identified as 'a very real worry', particularly with respect to researchers in sociology and political science who were 'not as numerate as their colleagues overseas and the gap is widening' (ESRC, 1987: 7). Just like Rosenbaum, the ESRC noted that the root of the problem was often that social science applicants usually had had little mathematics training in school; but the report was vocal too about the problem of *attitudes* pervading the sociological tradition: 'at worst some social scientists appear to show not only indifference but disdain for statistical training' and that it 'would be very dangerous for the future health of social science to allow this situation to continue' (ESRC, 1987: 7). With regard to the assessment of statistics and demography, the 1987 report differed little from Heyworth and Rosenbaum by pointing to the small, insufficient number of statisticians and demographers of outstanding quality; although they were producing work that was envied worldwide, lack of resources would 'severely hamper the ability to maintain this position in the future' (ESRC, 1987: 2). Another source of

worry was the extent to which the power of computing was being utilised in the social sciences, particularly sociology, as reported by the Computer Board for Universities and Research Councils (1970) and by the Review Committee on Sociology (1989). Although such worries were not explicitly related to the problem of numeracy, both were largely a product of an underlying attitude common among sociologists that it was best to avoid or disregard quantitative research and/or training.

Overall, it appears that every subsequent report was simply better at stating more clearly, or more vocally, the on-going problems identified by its predecessors – although progress was mentioned with regard to other developments, progress on the development of quantitative sociology was not reported. What, then, is the legacy of these reports? Why are they historically important if they repeat with different words and phrases the existence of one and the same problem?

Firstly, the reports show that, unlike the first half of the twentieth century when the direction of academic sociology was a matter of personal decisions of sociologists in a couple of departments, from 1940s onwards the overall academic characteristics of sociology became a matter for public enquiry and debate.

Secondly, the reports are important precisely *because* they repeat the same problematic issues, thereby indicating the existence of a consistent trend in British sociology charactersised by an inability to resolve the complex issues involved in the development of quantitative research and training *within* academic sociology.

Thirdly, they are important because their remarkable repetitiveness can be interpreted as a sign that the direction of their recommendations was consistently at odds with what social scientists thought or wanted for themselves – this is clear from the contrast between the discussions in the reports and the discussion of the nature and methods of sociology which was analysed in previous chapters. The problem of numeracy, although identified as a problem by those writing up the reports and those giving testimonies, was rarely, if ever seen as an urgent problem among mainstream sociologists.

The legacy of the reports is also important because they reveal the practical difficulties in incorporating the teaching of statistics into the social science curriculum. Although this thesis does not examine in great detail the complexity of the pedagogical issues involved in the incorporation of quantitative methods into

sociology, it has been my aim to show, that British sociology rarely reached the stage of attempting to tackle pedagogical issues because of a deeper-lying conviction that statistics was neither necessary nor desirable. The analysis of the reports is also vital because it shows, in contrast, that where such attitudes and beliefs about the usefulness of statistics in social science were not present as a barrier, namely among the social statisticians and a minority of statistically minded social scientists involved in the preparation of these reports, much more attention was paid to these practical issues[31]. Statistics has many potential areas of application and given that it was being adequately incorporated into other disciplines, notably the natural sciences and economics and in other social research institutes, solving the problems of getting it incorporated into sociology was, perhaps, not seen as urgent on the part of the statisticians. On the other hand, sociology in the post-war period received plenty of support and encouragement – it did not have to worry about proving itself statistically savvy. Allowing for some prejudice on *both* sides regarding attitudes to each other's disciplines, the analysis of these reports provides at least some support for the idea that, overall, it was the social statisticians and the statistically minded sociologists who employed a more sober, rational and practical approach to the issues involved in the relationship between both subjects by looking at the merit of statistical investigation beyond its mere application in surveys or other types of research:

> The statistician is a scientist or, more exactly, he is an expert in the methodology of science especially as it relates to dealing with uncertainty and variability. Since the social sciences abound in both there is a prima facie case for a substantial statistical presence. The possibility of social science thus depends on whether the phenomena of interest can be classified and quantified in a meaningful fashion and on whether the patterns and relationships observed are sufficiently persistent (Bartholomew quoted in Goldstein, 1984: 266).

Moreover, social statisticians have more often admitted that the divide between 'those who regard themselves as numerate and those who do not' within the social sciences is a problem that needs to be resolved and the 'hostility and

[31] For an extensive discussion of such issues, going into the more intricate problems in the relationship between statistics and sociology, see Volume 4, Issue 1 from 1974 of the International Journal of Mathematical Education in Science and Technology.

incomprehension' between both groups – to be eliminated (Goldstein, 1984: 262-3). Social statisticians were also more likely to admit that the problematic situation is a result of such attitudes coming from *both* sides:

> At one extreme the non-quantitative social scientist often tends to reject quantitative techniques on the grounds that reality is too subtle to be captured by the crudity of any measurements which could be made and that any attempt at measurement is distorting. At the other extreme, statistically oriented social scientists or statisticians sometimes seem to be claiming that even if the measurements are inappropriate, subsequent manipulation can be expected to extract something useful. Both these views have some substance (Goldstein, 1984: 262-263).

In contrast, sociologists commenting on views coming from the research councils and other official bodies, which formed the basis of many of the reports quoted above, were continuously expressing their discontent, attacking the councils and their recommendations as being more responsive 'to the needs of government than to a student audience'; modifying their approach 'only to make themselves available uncritically for the purpose of policy research' and promoting 'old-fashioned forms of research design; as a result, the SSRC was 'resented for the influence it does exercise by many sociology teachers' (Rex, 1978a: 415; Rex, 1978b: 296).

It may have been true that the position of the councils was skewed in one particular direction, but views like these are one-sided too, unwilling to admit that, if not totally true, the recommendations contained *some* truth. Lack of dialogue was at the heart of the problem. What the discrepancy between the views expressed in the reports and the views expressed in mainstream sociology discussed in previous chapters meant for sociology teaching in practice, and how both sets of views manifested themselves in the design of sociology curricula at the LSE and other universities across the country, is the subject of the following sections.

Sociology, Social Science and Administration (SSaA) and Statistics Courses at the LSE, 1895-1979

The character of academic sociology in Britain in the twentieth century is nowhere so clearly seen as in the development of sociology at the LSE. The LSE has, undoubtedly, played a vital role in the history of UK sociology as an academic subject. The very first academic courses under the name sociology were set up there in 1904. Soon afterwards, in 1907, the first British sociology professorships were established and from 1907 to 1920 sociology was taught as a special subject that was part of the BSc degree in Economics. Again, it was at the LSE that the first sociology degree was set up in 1920 and, until 1945, it was the only institution in which sociology was taught as an undergraduate degree (MacRae, 1953) and where a variety of social sciences could be studied extensively[32].

However, the influence of the LSE on the development of sociology in Britain, for a number of reasons, remained strong even after the post-war expansion. Since, in the early post-war period, the LSE sociology department was the largest sociology department in the country, it followed that many of the junior teachers recruited during the early fifties had been students at the LSE (Little, 1963: 68). In the same period, many of the newly established sociology departments in other institutions taught a LSE external degree. It is therefore reasonably justifiable to treat the results from an analysis of sociology at the LSE as representative of academic sociology as a whole; similarities of teaching trends in sociology at the LSE with that of other universities in the post-war period, as detailed below, provide additional support for the contention that the LSE is a strong representative example.

The analysis of the teaching of sociology at the LSE examines whether sociology in the UK has been taught as an 'arts' subject, placing emphasis on the development of skills in analytical thinking and writing; or whether it has been taught as a 'science' subject, with emphasis being placed on empirical research skills, including statistical skills. To answer these questions, the analysis focuses on the types of courses, according to the skills they teach, that were predominant in sociology at the LSE and also on whether any important changes occurred in this respect in the period between 1904-1979. The Social Science and Administration (SSaA) courses at the LSE (1912-1979) are also examined in order to shed light on

[32]A possible exception is Liverpool where a School of Social Science was established in 1917, a Chair in Social Science - in 1922 and a BA honours degree - in 1926 (Fincham, 1975: 101).

the relationship between the two subjects – sociology and social science and administration – and find out how they influenced one other and whether they were teaching the same or different skills. Finally, the analysis looks at the particular methods courses, including statistical courses, which were available to sociology and other social science students.

The data for this analysis come from the LSE calendars from the period 1895-1979. Each calendar contains a list of courses sorted by subject area. The lists contain the following information about each course: name, course convener, the term in which the course is given; timetable of the lectures; classes; syllabus and recommended literature.

All courses are given equal weight, regardless of the length of the period in which they were taught. This is because the purpose of the analysis is to establish what *types* of courses, according to the skills they teach, were predominant. If the focus of the analysis had been to examine what sociological *fields* were predominant in the curriculum, then it would have been important to take into account the duration of course existence.

Classes are excluded from the analysis as almost all classes were provided as a supplement to main courses and adding them to the analysis would not have changed the results.

This analysis is quantitative in the sense that it uses numbers to describe an historical trend; it is, however, not strictly statistical, since it does not employ statistical techniques to test the significance of the findings.

The findings should be treated with caution, since examination formats remain unknown, making it impossible to draw any conclusions about the specific skills in which students were *examined*. Also, as the data provide no evidence as to the quality of teaching and course organisation, I can draw no conclusions in this regard; nor am I able to tell if the courses covered *all* the material in the syllabus. There is also no record of how many students took a particular course or passed and with what grade.

The analysis of sociology courses makes an important distinction between courses which *aim* to teach students practical empirical skills; and courses which do not, *although they might discuss published empirical research and the skills employed in that research*. The classification of sociology courses into types was made using the "Syllabus" section and the "Books recommended section" and can be seen in Appendix I, Table 16. Statistics courses have never been listed in the list of sociology courses but those statistics courses which were part of the sociology degree have been incorporated into the analysis in order to analyse the relationship between sociology and statistics.

I. Sociology as a Special Subject in the BSc Economics, 1904-1923

Sociology was established as an honours subject, optional for the BSc in Economics and BA in Philosophy in 1904-5, thanks to the benefaction of James Martin White, a Scottish businessman. Sociology courses then included ethnology, social psychology and philosophy, the comparative study of social institutions and social evolution.

It is not entirely clear how the sociology subjects were chosen. Fincham (1975: 32) argues that because courses on statistics and social administration were taught in the so-called School of Sociology[33], the precursor of the SSaA department, the emphasis in sociology *had* to fall on something else; as well as fit with Martin White's own vision of sociology.

This seems a reasonable explanation, assuming that such a decision would have been made with a view to determining how best to promote sociology to prospective students. However, lack of people with statistical and social administrative skills available to teach sociology in the LSE at this time is also an important factor that should be taken into account. Although the 1904 negotiations at

[33] The School was led by Charles Loch of the Charity Organisation Society. It is not clear why they chose to call it a 'sociology' school, but the School had very little in common with the Sociological Society and it is possible that they used the word sociology differently; in fact, when Branford, chief organiser of the Sociological Society, gave a paper in front of the School of Sociology in 1904 on the founders of sociology, he received a very bad reception (cf. Branford, 1904a).

the LSE were important in deciding what *exactly* was going to be in the sociology curriculum, the available options were pre-conditioned by an assumption of what sociology is **not,** and this assumption had been formed long before. As I showed in Part One, by 1904, it was clear that sociology was **not** going to follow in the statistical and social reforming tradition that had developed in the UK during the nineteenth century. It is hard to imagine that if the so-called School of Sociology did not exist, statistics or other more practically oriented subjects would have automatically become part of the sociology curriculum.

There were few sociology methods courses within sociology in this period. 'Logic and scientific method', although not always listed under sociology, was an integral part of the curriculum and was the only subject in the 'Principles of method' exam paper. The course taught the philosophy of scientific inquiry rather than the teaching of *skills concerned with how to do* scientific enquiry.

An attempt in this direction was made in 'Methods of Social Investigation', a course set up by Beatrice Webb, which taught how to pose a hypothesis, collect data, conduct interviews and examine archives. The course, however, did not last long and the teaching of practical skills in scientific research languished. Only 1 out of 47 courses in this period taught some empirical skills; the rest were courses teaching the study of society using theoretical or philosophical approaches (Appendix I, Table 18).

II. The BA Sociology Degree, 1924-1952

During the decade beginning in 1910, there had been discussions about the possibility of establishing a sociology degree. This option was debated at a meeting of the Board of Studies in 1914 and a number of problems were highlighted (cf. Fincham, 1975). The subject was deemed 'too difficult' – it was noted that students in BSc Economics with special subject 'Sociology' performed rather badly and had to spend disproportionately more time working on sociology in order to perform well, which, in turn, discouraged future students from specialising in sociology. It was recognised that the subject was vast and although some steps had been taken to try to narrow down its focus, they had not been very effective and the Board found it

difficult to decide how best to address the situation. It was suggested that the root of the problem was that the students did not have enough time to focus on sociology, which would only be possible if sociology were made a degree in its own right. This was also seen as a logical solution, since sociology was viewed as "an attempt to conceive the social problem as a whole" (Fincham, 1975: 51) in which economics was only a special field. Since it would have been more difficult to reform the economics degree, the plans for a new degree went ahead and the first examination for BA Sociology took place in 1924. 'At this stage, the degree writers seemed to be looking at the subject very much as an art subject; no economics or statistics were included' (Fincham, 1975: 54). After the establishment of an independent sociology degree, in the 1930s 'sociology' was being taught only at the University of London – there were sociology courses at Bedford college but the only place where professed sociologists worked was the LSE (Marshall, 1936: 30). There was teaching of social science more generally going on at other universities, such as Liverpool, Edinburgh, Manchester, Birmingham, Leeds and Nottingham but this did not include the material taught as 'sociology' at the LSE, which at the time was predominantly philosophical.

Until 1952, when the BA Sociology degree was again re-organised, it proved more popular with women than men; and was less popular than the BSc Economics with Special subject Sociology (Marshall, 1936). In addition, while economics students specialising in sociology were taught statistics as part of their economics training, the BA Sociology did not include any statistical courses.

In 1924-1952, the number of purely theoretical courses increased – from 38 to 47 (Appendix I, Table 18). However, the proportion of these courses diminished, from 80% to 58%, suggesting decreasing influence. In addition, the majority of the courses in this category were replaced by new, but similar courses. These changes can be explained by the growing influence of Morris Ginsberg, a disciple of L. T. Hobhouse who replaced Hobhouse as Professor of Sociology after Hobhouse's death in 1929. Ginsberg's courses differed little in terms of content and approach – with the same emphasis on the comparative study of social structure, social evolution, social philosophy and ethics.

New courses were also introduced by the American sociologist Edward Shils, who joined the LSE sociology department in 1947, bringing in the American advances in theoretical sociology. New staff and new courses, however, did not mean much change in the general approach to the study of society or the skills taught to sociology students.

The change came from a different direction. More courses dealing with contemporary issues were added to the sociology curriculum after 1945. These courses were introduced into sociology from the SSaA lists of courses, and together with the sociology courses on contemporary issues, they constituted 24.6% of all courses. But although the range of topics that counted as sociology was extended, the broadly theoretical approach towards the study of society and the teaching of theoretical/analytical skills remained predominant.

One important change was the introduction of a number of methodological courses, including some statistical courses (ca.15% of all courses). Shils introduced a course on 'Sociological Research', which included 'the assessment of the techniques and results of modern sociological investigation' and focused on 'the relationship between empirical social research and sociological theory' (LSE, 1947). It is doubtful that this course taught any practical empirical skills; however, judging by the description, it is more likely that it taught critical discussion of existing methods.

A course on population was taught in 1932-1937 by the staff working in the newly established department of Social Biology. It is likely that this course would have required some statistical knowledge (in vital statistics, for example) but there is no indication as to whether the course involved the teaching of practical statistical skills.

A step in this direction was made in 1951-2, when statistical courses on elementary statistical methods, survey design, demography and sources of statistical data were introduced. These were not new courses, specially designed for sociologists; they were courses that were already part of the BSc Economics and BSc Commerce. The teaching of statistics to sociologists in 1951-2 and 1952-3 remained at an elementary level: not going 'beyond the simplest ideas of sampling and correlation' (LSE, 1951: n.p.).

The first statistical course for sociologists was introduced in 1947 by Claus Moser. It taught 'the planning of social investigations', 'methods of collecting information', 'practical sampling techniques', 'contemporary social work' (LSE, 1947). The course taught relatively sophisticated 'practical sampling techniques', based at first on Yates (1946) and later on Moser (1958). There was a considerable amount of mathematics involved in the course, but it is difficult to judge how the course was perceived by sociologists and sociology students at the time. The course ran for two decades (1947-1971) before coming to an end. It may be speculated, that its rather narrow focus somewhat distorted the image of statistics within sociological circles, making it look as if statistical knowledge is not useful for anything else except surveys. Had the statistics curriculum perhaps put more emphasis on the benefits of acquiring a basic understanding of the statistical characteristics of the social world as whole, which is useful whatever the research method or question, a more widespread appreciation of statistics among all sociologists might have resulted.

Two optional courses on demography were also introduced. These courses required working with official statistics and already collected data on vital statistics, which was perhaps enough to inspire students who already had an interest in demography. Although these courses provided a good overview of various types of statistical data and how it could be used for analysis, it is not clear how they would have been perceived by students with no or very little interest in statistics or mathematical knowledge. There was an additional course on demography called 'Mathematics of Population Growth', but this course was for economics students only.

The only methods course taught by a sociologist was Ginsberg's course 'The Theories and Methods of Sociology' (1933-1972), which only included discussion of the comparative and historical methods, with no practical work. Methods were a topic of philosophical discussion rather than a set of techniques that require practice. Fincham's examination of this period confirms these conclusions – 'the impression was that non-quantitative subjects predominated and that theory took precedence over method in the syllabus' (Fincham 1975: 96).

III. Sociology Courses, 1953-1979

In 1953 there were 15 universities where sociology/social studies were being taught and where social science research was being done; the LSE still retaining its foremost position in the education of sociologists (MacRae, 1953). MacRae's report shows that of these 15 universities, 11 taught or carried out research using some statistics and 4 did not (Appendix I, Table 15). Could this be an indication that the teaching of statistics became an essential feature of sociological teaching once the expansion was under way? Not necessarily, as the scale, quality and type of this statistics provision remains unclear and highly questionable – the information in MacRae's report is not easily comparable, especially as it also covers other subjects such as psychology and social anthropology; and since the data give insufficient clues as to how much statistics was offered in each university, the analysis makes no assumption as to the level of sophistication or quality of the statistical/survey teaching in the universities in which this was practised. Examining the LSE sociology courses in this period (Exeter, Hull and Leicester taught the LSE external degree at the time) would therefore allow us to establish with greater confidence the extent to which the mere presence of 'statistical' or survey elements in the curriculum meant that sociology students were being taught how to collect and analyse statistical data.

The LSE sociology degree regulations were changed in 1952. Two sociology degrees were now available: the old BA degree, which required knowledge of Greek and Latin; and a new BSc degree in Sociology, which did not. The two degrees were otherwise the same and included three specialisation options. In the first option, the emphasis was on 'pure' sociology, and the teaching of the old theoretical courses, catering for those students who would later become teachers in sociology in higher education institutions and academic researchers. The second option, in which the emphasis was on the teaching of SSaA courses catered for well-qualified social administrators. And the third, in which the emphasis was on social anthropology was designed to prepare potential candidates for Colonial Service.

Sociology's scope was expanding but this did not result in many changes in the *skills* that sociologists taught their students. The proportion of the purely theoretical

courses decreased a little, from about 58% to about 52% (Appendix I, Table 18). But regardless of the substantial changes in teaching staff, university expansion and the social conditions that had apparently made sociology more popular, pure theory courses were still prevalent.

The proportion of courses on contemporary conditions increased noticeably after the reconfiguration of the degree – from about 15% in the period 1904-1923, to 36.5% in 1953-1979 (Appendix I, Table 18). But these courses too, taught little, if any, empirical skills.

In addition, there was a decrease in the proportion of methods courses of *all kinds,* including a slight decrease in the proportion of statistics courses (Appendix I, Table 18). This overall decrease, however, reflects a diminishing proportion of methods courses relative to the overall increase in sociology courses, not an absolute decrease of the number of methods courses. While the scope of the sociology curriculum was expanding in order to accommodate a growing interest in contemporary issues, the methods courses expanded very little.

All three options within the sociology degree included statistical courses, as the new syllabus was designed to provide fuller opportunity for training in quantitative methods (Board of Studies in Sociology, 1947-9). But a closer inspection shows that these courses had a limited impact. Moser's course continued to run unchanged until the 70s, under a new name – 'Survey Methods of Investigation' – that corresponded more accurately to its narrow focus, compared to the newly introduced general statistical courses – 'Social Statistics', and 'Statistical Methods (sociology)', which were designed specifically for sociologists.

'Statistical Methods (Sociology)' was the major statistical course in the degree. Compared to previous courses, it was a more advanced course, which in the 1950s taught frequency distributions, averages, elementary theory of regression and correlation, background of sampling theory, calculation of sampling errors and design of samples. It was also a more practically oriented course that paid attention to the application of statistical methods to sociological problems and so it could be seen as a positive effort to bring sociology and statistics closer together.

In the period 1966/7-1968/9, however, the course was divided in two – one course for students taking the 'pure sociology' and 'social anthropology' options of

the sociology degree, which included only the teaching of basic descriptive statistics; and another for the students taking the 'social administration' option, which taught basic descriptive statistics *and* more sophisticated material, such as statistical inference, correlation, regression, significance tests etc. A joint course for all options was again introduced in 1969, but the more sophisticated part of the course was dropped.

The number of demography courses increased. These were taught consistently between 1953-1979 and were part of a special set of courses taught by a strong team of demographers (Glass, Grebenik, Langford, Hajnal). However, while sociology students were taught the basic, descriptive, demographic courses, the more sophisticated courses such as 'Mathematics of Population Growth' and 'Mathematical Techniques for the Manipulation and Analysis of Demographic Data', which taught the essentials of how to *do* demographic research – were offered only to economics students and students specialising in demography. Consequently, the demography courses came to represent yet another element of sociologists' 'general education', broadening their worldview but not contributing much to the acquisition of practical quantitative skills.

Overall, the courses teaching theoretical analytical skills have been dominant (59.8%) in the period 1904-1979 (Appendix I, Table 18). If we add courses on contemporary issues and philosophy of methods or science, which teach similar theoretical, analytical, but not empirical, skills, then this figure rises even further (Appendix II, Table 18). It could be argued that this merely reflects the fact that there were many courses on a great variety of special topics that had to be taught in sociology and that are included here. This is true; but it nevertheless shows that sociology has been more interested in teaching 'topics' and that the teaching of empirical skills has not been a priority[34]. A few empirical courses, including statistical courses were offered on a regular basis in the post-war period, but they did little to change the overall character of the subject. As far as statistical skills are concerned, it is unlikely that statistics courses would have produced cohorts of

[34] Sociology is not unique in this respect. It is unlikely that the teaching of skills in social anthropology, or politics was much different. A contrast could be made with economics, but only with regard to the teaching of statistical skills.

statistically literate sociologists since the majority of them provided sociology students with only elementary knowledge.

The Teaching of Social Science and Administration at the LSE, 1912- 1979

The teaching of SSaA provides a useful opportunity to examine how sociological teaching at the LSE was different, or similar to, the teaching in the broader spectrum of subjects under the title 'Social Science'. The teaching of SSaA independently from sociology is one of the manifestations of the parallel but separate tradition of empirical social enquiry that existed in Britain throughout the twentieth century. Knowing how this developed will afford us a better understanding of the factors that sustained this divide.

As with the analysis of sociology courses, I have relied on the information in the LSE calendars under the headings 'Syllabus' and 'Recommended Reading'. The rules for allocating the SSaA are the same as the rules for allocating the sociology courses described in Appendix I, Table 16.

I make a distinction between Theoretical/philosophical/historical course for SSaA only and Theoretical/philosophical/historical course for SSaA and Sociology to determine if and, if so, *when* sociology students became more engaged with the contemporary issues that were being taught in the SSaA; through what type of courses this was achieved; and whether this provides us with an indication of any changes in the character of sociology (cf. Appendix I, Table 19).

The SSaA department was established in 1912 and was oriented towards the improvement of social conditions and devoted to preparing students to 'engage in many forms of social and charitable effort' and to give students 'first-hand experience in social work' (LSE, 1912). This was organised through the department's close links with associations such as the Children's Care Committee, Skilled Employment Associations, Labour Exchanges and the Charity Organisation Society. The SSaA department was not as narrow-minded and 'practically biased' as is generally portrayed (Fincham, 1975: 98), at least not as far as their curriculum is concerned. Alongside courses on existing social conditions, the SSaA department

taught courses on social philosophy and theory, the aim being not to teach students 'to theorise, but to deepen their intelligent interest in everything connected with their subsequent and practical work'; they believed it was 'necessary that students desiring to take the full course should possess a good general education' (LSE, 1914).

What was the relationship between the SSaA and the sociology department? The SSaA's view of sociology has been remarkably consistent. As the 1914 calendar suggests, sociology was perceived as fulfilling a 'general education' role, because sociology was associated with theorising about the social world; in a similar vein, in 1979, John H. Smith, a student at SSaA and later a lecturer there, recalls that in the 1950s-1960s 'less emphasis was placed on sociology, at any rate in its more theoretical and suspect manifestations' but, he did not hesitate to emphasise, 'the empirical study of contemporary social problems was included' (Smith, 1979: 444). Throughout the twentieth century the SSaA taught about the nineteenth century initiatives that had been quickly dismissed as 'not sociology' within sociology itself – the statistical societies, the Poor Laws, the charities etc. The SSaA's wide scope and close engagement with contemporary social issues could perhaps explain why the SSaA was attended by much larger numbers of students and candidates for qualifications than Sociology, even though it only offered a certificate (Fincham, 1975: 59).

It has been argued that, over time, the prestige of the SSaA department increased – partly due to the fact that it was often approached by the government on various issues – while the prestige of the sociology department did not. Fincham explains this by suggesting that the prestige of the SSaA department had 'the effect of attracting away from sociology men who might have become professional sociologists, but who wanted to combine an academic role with social action' (Fincham, 1975: 60). However, it appears that Fincham was misdirected to this untenable conclusion after reading Abrams' (1968) on the origins of British sociology, in which he blames sociology's early lack of successful development in this country on the success, or simply the presence, of other social science institutions. It is reasonable to assume that if students had an interest in practical, as well as theoretical social science, they would apply to the SSaA, not the Sociology, department. But it is far from reasonable to assume that one department was 'stealing'

students from another, given that both departments offered very different courses and a substantially different education.

To analyse the make-up of the SSaA courses, I have divided the material into two periods – 1912 to 1950 and 1951 to 1979. In 1951 Richard Titmuss became head of the SSaA department and his arrival proved to be a turning point in the history of SSaA. Together with his colleagues, Titmuss revamped the SSaA and, arguably, enhanced its overall reputation, its relevance and public role. Titmuss and his colleagues were heavily involved in the 'building' of post-war welfare state institutions in the UK and at the LSE they provided the necessary knowledge of the principles and mechanisms which were essential for the welfare state to work.

Two trends are noticeable when we compare the two periods (Appendix I, Table 19). Firstly, over time, the SSaA became less philosophical and more oriented towards the teaching of contemporary issues: in 1912-1950, nearly one third (27%) of SSaA courses were purely philosophical; in 1951-1979 this number almost halved. Although predominance of purely theoretical/historical courses in the first half of the twentieth century is observed in *both* sociology and SSaA, the focus of this type of course differed in both subjects – the SSaA courses focussed on economics theories and social history, while sociology focussed on social evolution, principles of social development, religion etc.

Secondly, in 1912-1950, only 2% of SSaA courses were offered to sociology students; in 1951-1979 – more than half of SSaA courses (55.6%) were offered to sociologists. The bulk of these courses (45%) were on contemporary issues (Appendix I, Table 19).

The amount of methodology teaching in both these periods was very low and the level was elementary. Few statistics courses were available which taught basic descriptive statistics and none taught data *analysis*. But while the SSaA courses were no more empirical in their approach to methods and methods teaching than sociology courses, they provided professional and practical skills via placements, internships and direct communication with staff in government departments and other agencies.

Thus, the SSaA department, and what was being taught there, played an indirect but significant role in the development and understanding of sociology in Britain. But the sociology and SSaA departments remained separate, even after

nearly half of SSaA courses were incorporated into the sociology curriculum, which suggests that, at the very least, the two subjects had a distinct understanding of themselves and of each other. The SSaA department represented the British tradition of investigation into the social conditions, largely brought about by the Industrial Revolution, that began in the nineteenth century with the work of Chadwick, Booth, Rowntree and other social reformers – a tradition that has never been recognised as sociology or sociological enough, perhaps because it lacked the 'more theoretical and suspect manifestations' Smith (see above) recognised as characteristic of 'true' sociology. But the SSaA department did more than produce people to work in governmental and public institutions: 'if you asked what we taught, the answer would not be social administration'; their teaching was focused on 'the significance and interdependence of economics, social history and psychology' (Smith, 1979: 444).

Although on the surface the incorporation of SSaA courses into the sociology curriculum might look like the long-awaited merging of two traditions, the two subjects remained divided – in the topics they taught, the level of abstraction at which they taught them and in the fundamental attitude they had towards the study of society. In SSaA, students had to study contemporary social conditions and the working of institutions in order to understand how society worked; in sociology, students had to grasp the theories and principles of social development. And, even though the incorporation of SSaA courses into sociology was not very successful in merging the two traditions of social inquiry, it seems reasonable to assume that one effect it did have was to contribute to the myth that sociology in Britain was somehow 'positivist' in the early post-war period, where 'positivist' is understood as having little engagement with abstract theory and focus on 'the facts of society'.

The Teaching of Statistics at the LSE, 1895-1979

If statistical courses in sociology and SSaA were present in certain periods but were also basic in terms of the skills they taught, then in which subject and to what students were more sophisticated statistical skills taught? And what, if anything,

could the provision of statistical courses in other subjects tell us about the provision of statistics subjects in sociology?

The LSE introduced statistics courses when it was first established in 1895. Up until 1920, they were listed under 'Economics'; from 1920 onwards, they were listed in a separate section, together with mathematics. In 1958, computational methods were added to the section, gradually becoming more and more specialised. In 1958-1961, among the computational methods courses, there were courses on advanced statistics and mathematics which were recommended to economics students specialising in statistics and which have therefore been included in the analysis. From 1962 onwards, the courses listed under computational methods included only specialist computing and software courses, and those have been excluded from the analysis. It is important to note that while computing courses very quickly became an intrinsic part of the education of economics students, they were not recommended to sociology students.

To analyse the types of statistical courses offered, I use the information in the calendars about content as well as the recommended reading list and the degree/subject of which they are part. This allows us to distinguish between statistics courses for sociology, as opposed to economics, students. Details about this distinction are available in Appendix I, Table 17.

Less than 10% of all statistics courses offered at the LSE between 1895-1979 were available to sociology students (Appendix I, Table 20). We saw earlier on that statistics courses represented only a minority of the total of *sociology* courses. Now we see that the statistics courses for sociology students represented only a minority in the total of *statistics* courses. It is very unlikely, therefore, that the number of statistics courses in sociology was small due to a lack, more generally, of statistical courses or suitable resources.

We also saw that the majority of these courses appeared in the late 50s and taught elementary[35] statistical techniques, mostly data *collection*, with little evidence to suggest that students were being taught how to *do* statistical *analysis*. In comparison, the statistical courses for economists taught students how to do *data*

[35] 'Elementary' is relative to the level of sophistication of the courses available to economics students; not relative to a 'universal' standard.

analysis using regression models, multiple partial correlations, probability theory, different types of distributions etc. In addition, there were statistics courses devoted to specific branches of economics; for instance, econometrics, national income and capital, and macro-economics, bridging the gap between the two subjects. Extra mathematics courses were also offered. It is clear, therefore, that a vast amount of statistical knowledge and teaching, especially in data analysis skills, was available at the LSE, but largely being offered to economics students and not to sociology students. Without being taught more sophisticated practices, it seems unlikely that sociology students would have benefited very much at all, even from the elementary techniques they were being taught.

The wide availability of statistics courses offered to economics students shows that the necessary resources were in place to do the same for sociology students but the fact that such courses were not offered to sociology students suggests that the reasons why this was not the case ran much deeper. Arthur Bowley, the first UK Professor of Statistics, who created and sustained statistical teaching at the LSE during the first half of the twentieth century, suggests that one deep-lying reason is the way sociology had defined itself, as something that includes 'the knowledge of all groups of actions and relations past of present, measurable or not' (Bowley, 1923: 3). One possibility was to talk about 'modern statistical sociology' but that was different from saying that sociology, in itself and as whole, was a statistically oriented subject.

Furthermore, when Bowley accepted the professorship in 1914, he "set to work to find out what "Statistics" meant as a branch of economics or mathematics" (Bowley, 1945 quoted in Fincham, 1975: 54) – he does not mention sociology. In his approach to teaching and choice of topics, Bowley remained economically, not sociologically minded, even though he covered a wide range of material, including the teaching of mathematics, official statistics and history of statistics (Bowley, 1922-23). But if Bowley's lectures were to be compared to Hobhouse's or Ginsberg's, it would be very difficult to even imagine that they would be teaching the same subject. Sociology and statistics, from early on, were defined as incompatible – a view, which could hardly have been changed by providing more

and more suitable resources, such as computers, at the disposal of sociologists and sociology students.

This is not to suggest that statistics was a narrow specialty. In the first half of the twentieth century, the great majority of the statistical courses were courses suited for *all* economics students; not only those specialising in statistics. More specialist courses developed only during the 50s when it became increasingly necessary to have specialist knowledge in the more sophisticated techniques of analysis and computing skills[36].

The Teaching of Social Science at the LSE: Summary

The analysis of undergraduate sociology teaching at the LSE throughout the twentieth century shows that sociology was taught as a subject whose aim was to provide a general or liberal education. As a result, training in statistical or any other quantitative methods was continuously perceived as unnecessary or irrelevant; and where statistics was taught, this was done merely to *introduce* students to the existence of the methods and rarely to train them to do quantitative research. These results support the argument developed in previous and subsequent chapters that, at its core, the relationship between statistics and sociology in Britain has been shaped not by practical concerns or by rational or deliberate discussion but by implicit, and even unequivocal, attitudes among course conveners, and perhaps even among the students themselves, as to what sociology is and is not. The availability of good quality statistics courses open to economics students but not to sociology students is only one indication that the lack of more comprehensive statistical provision in sociology was not necessarily a matter of lack in expertise or opportunity. Such attitudes, prevalent at the LSE, both reflected and reinforced general attitudes and

[36] Thorough examination of the computing courses, which first appeared in the 1950s, show that they were available primarily to economics students specialising in statistics. The lack of proper computing training, or even access to computing machines, which was necessary for sophisticated statistical analysis, only exacerbated the view among sociologists that statistics is not 'a sociological skill'. First attempts to turn computing into a universally applicable skill, and draw attention to its general advantages, were made in the 70s; but these attempts fell on deaf years in the sociological community (cf. Computer Board for Universities and Research Councils, 1970 for a more detailed discussion of the role of attitudes to computing prevalent in social science).

assumptions in the wider sociological community that persisted throughout the period studied and which continue down to this day. To obtain an even clearer view of the extent to which the teaching trends at the LSE were present in sociology teaching to undergraduate students more generally, the next section turns to examining the teaching of methods courses including statistics and survey courses to undergraduate sociology students in *all* universities in the 1960s and 1970s.

The Teaching of Methods Courses, including Statistics Courses, within Sociology First Degrees in British Universities, 1967-1979

Although the LSE data provide a relatively representative example of the teaching trends in twentieth-century British sociology, an analysis of the teaching trends in sociology across the whole country would be preferable in order to situate the LSE findings within a general picture. The only accessible and reliable sources of raw data on sociology curriculums, including sociology methods curriculums, are the Peel (1968) and Wakeford (1979) collections of syllabuses and sociology degree summaries in the student guide *Which University/Which Degree* (1969-1979). While the Peel's and Wakeford's collections were discussed at the 1981 BSA conference on Research Methodology, this is the first time that the data they contain is analysed.

The Teaching of Methods in Sociology First Degrees across the UK According to Peel (1968) and Wakeford (1979)

John Peel's syllabus collections was compiled for the 15th conference of Teachers' Section of the British Sociological Association (BSA) in 1968. The Teachers Section of the BSA flourished in the 1960s – it was a period of continuous expansion of sociology courses and degrees in the universities, going hand-in-hand with an increased interest in the teaching of sociology.

Peel collected syllabuses of methods courses from 26 sociology departments. The present analysis is based on data from 25 sociology departments, since Oxford

University, which only taught courses to postgraduate students, has been excluded. Peel's compilation contains syllabuses of both theory and methods courses[37] but the present analysis focuses only on the methods course syllabuses.

Wakeford's collection was conceived as a continuation of Peel's and was prepared for the session *The Teaching of Methodology in Sociology* in the *Conference on Methodology and Techniques of Sociology*, held in Lancaster in January 1979.

Wakeford collected data from 53 universities and polytechnics (out of the 77 he initially approached). From these 53 universities, data from only 49 have been analysed here. Data from the London Graduate School of Business Studies, Imperial College, Manchester Business School and Oxford University have been excluded since these institutions taught only postgraduate courses.

The structure of Peel's and Wakeford's collections of syllabuses is similar and allows for a reasonable comparison between the state of methods teaching in sociology departments in 1968 and in 1979. In both collections, some universities did not strictly follow the structure for reply suggested by Peel and Wakeford but, with a small number of exceptions, they have all provided the necessary material for the present analysis, including syllabuses, reading lists and sample exam papers.

The purpose of the analysis of the collections is to compare in a systematic way the research methods that were taught in sociology undergraduate methods courses in the late 60s to the late 70s, particularly the teaching of statistics. This is organised around three main questions.

First, how many universities in the UK offered *at least* one methods course that included statistical training in 1967 compared to the situation in 1978? The aim is to find out how many universities regarded the teaching of statistics (regardless of quantity and level of sophistication) as a necessary component and whether, between 1968 and 1979, the number of universities where statistics was taught diminished. To answer this question, the total number of methods courses offered at each university was counted. Where courses have the same name but are separated between

[37] Attached to the Peel collection is another collection of syllabuses on the 'Details of courses mainly concerned with Social Structure and Comparative Social Institutions in British Universities and Colleges'. This collection was compiled in 1962 by John Peel and John Wakeford. Data from it have not been analysed here.

academic years as in, for instance, Research methods I, Research methods II and Research methods III, they have been listed as three courses.

The number of methods courses in each university that contain *some* statistics teaching were also counted. At this initial stage, no differentiation was made between the amount of statistics or level of sophistication of the statistical material in the courses; but courses which only contained a discussion of the survey method were not considered as providing statistical training if there wasn't clear evidence that the teaching of this method was accompanied by the teaching how to do statistical analysis of survey data.

Lastly, the number of universities which offered *separate* statistics courses were counted; it being reasonable to assume that a separate statistics course would provide students with the opportunity to acquire more advanced skills and longer time to practise them.

Second, what methodological techniques are taught in undergraduate methods courses? After a preliminary examination of the syllabuses in both collections, I made a list with the following methodological techniques: statistical techniques and computing; survey method and questionnaires; observation (including participant observation); interviews; documentary and content analysis; experiments; and philosophy of science/method. The universities in which methods courses discuss 'positivism' were also counted in an attempt to trace the popularity of the usage of this term and to try to gauge how influential the debate surrounding positivism had been on methods teaching. No inferences were made about the quality of teaching of methodological techniques; this part of the analysis is limited to indicating whether particular techniques were *mentioned* in the syllabuses. No comparison of the teaching of methods between universities was made, except in the case of statistics and computing, where the analysis distinguished between universities that taught courses containing little statistics (with no sign of practical work exercises) and those where sociology methods courses contained advanced and more intense statistics teaching (usually indicated by the teaching of more sophisticated techniques, practical workshops in computing and the use of more advanced statistical textbooks). It could be hypothesised that the teaching of statistical techniques and computing, compared to the teaching of 'qualitative methods' in this period declined

due to increasing popularity of feminist and 'anti-positivist' approaches to sociology, which claimed that statistics was not an adequate tool for doing sociological research.

Third, what was the level of sophistication of the teaching of statistics in the universities where statistics was taught? Did courses involve the teaching of practical statistical skills, or did they limit themselves to introducing the students simply to the existence of the method? In this part of the analysis, *only* statistics teaching in different universities was compared. Three main criteria were used to assess the level of sophistication of statistics teaching in methods courses. The types of statistical techniques (e.g. descriptive, inferential) that were mentioned in the syllabuses were examined – for instance, more sophisticated statistical modelling, inferential statistics etc. are taken as a sign of deeper engagement with the subject. I also noted the statistical textbooks that were mentioned in the syllabus and, from an examination of the contents of those textbooks, it was possible to determine which would best facilitate the teaching of practical statistical skills. Lastly, computer usage was examined, which has been taken as a sign of a more sophisticated level of statistics teaching.

Although the contents of the syllabuses is a reliable indicator of what was being taught on a particular course, it is not a guarantee that all of the mentioned material was actually taught. The results of this enquiry are conditioned by how much universities chose to report, what they chose to highlight in their syllabuses and how they described it. Any conclusions from this analysis are limited to the period under investigation. It is, of course, possible that new trends in the teaching of methods in British sociology emerged after 1979. However, no systematically collected original data on sociology methods teaching exists for the period after 1979; acquiring such data now would be an extremely difficult, if not an impossible, task.

Methods Courses in Sociology First Degrees, 1967-1978

The number of methods courses expanded nearly three-fold between 1967-1978[38] (Appendix I, Table 21). This was partly due to the expansion of the number of higher education institutions teaching sociology in this period, and partly due to the fact that the number of methods courses on offer within universities also grew – the vast majority of universities in 1967 offered just one methods course, by 1978 many offered two or more. So it is clear that by 1978, the teaching of methods had become an essential part of the undergraduate sociology degree.

The number of methods courses that taught statistics as a proportion of the total number of methods courses reported decreased slightly, from 67.9% to 56.1%, between 1967 and 1978 (Appendix I, Table 21). There was also a relative decrease in the number of separate statistics methods courses (Appendix I, Table 21). Does this mean that statistics was becoming a less popular subject in methods classes? Not necessarily. By 1978 the total number of methods courses *within* each university had expanded while the number of methods courses teaching exclusively statistics remained stable. This suggests that the general expansion of methods courses was aimed at establishing new courses devoted to the teaching of methods other than statistics. Therefore, the drop in the proportion of methods courses which contained statistics is best explained not as a result of sociology departments deciding *not to teach statistics*, but rather as a result of their decision *to expand methods courses in other areas and their ability to put this decision into practice*.

This assumption is further supported by the fact that the proportion of universities teaching *at least one* methods course that contained statistical training changed very little in this period: in 1967, 76% of universities taught at least one methods course that contained some statistics while in 1978, this was 75.5%. The proportion of universities teaching at least one *separate* statistics course also shows little change: 28% in 1967 as opposed to 26.5% in 1978 (Appendix I, Table 22).

It is extremely difficult to estimate how much statistical training might be regarded as being 'enough' to produce statistically literate sociologists in this or any other period. From an historical point of view, the important result that arises from a study of this vital period of consolidation of academic sociology in Britain, is that

[38] Peel's and Wakeford's collections were published in 1968 and 1979, respectively. The syllabus data, however, refer to methods courses taught in the academic year, previous to the publication of each collection, i.e. 1967 and 1978.

sociology students were not deprived of opportunities to learn *some* statistics as part of their undergraduate degrees; more importantly and, perhaps surprisingly given common assumptions about what was happening during this period, those opportunities did not diminish in any way. The evidence shows that there was virtually no change.

Methodological Techniques in Sociology First Degrees, 1967-1978

There was little change in the overall amount of teaching of statistics and computing between 1967-78 – 76% of universities in 1967 taught statistical techniques, as opposed to 72.9% in 1978 (Appendix I, Table 23). We get similar results when we restrict the comparison to those universities which taught statistics and computing at a more practical and advanced level – in 1967, only 48% of universities taught advanced statistics, as opposed to 54.2% in 1978. This shows two things: firstly, that the continued academic growth of sociology that took place between 1968 and 1979, notably the rise of the polytechnics, was not accompanied by a decrease in the teaching of statistics in sociology methods courses; and secondly, that if the rise of feminism and the spread of debates on 'anti-positivism' had any effect at all on the character of sociology in the UK (for further details see Chapter Thirteen below), this effect did not spread as far as the teaching of methods to undergraduate sociology students. If these movements did indeed nurture any scepticism, even hostility towards statistical methods, the practical effects of such attitudes lie outside the circles of the research methods teachers and their classrooms.

An overall stability is further manifested by the lack of change in the proportion of universities which taught the survey method as part of their methods courses – 92% in 1967 as opposed to 89.6% in 1978. The lack of change in the proportion of universities teaching the survey method in sociology methods courses is remarkable given that *during the 70s* it was a common belief that British sociology had already gone through 'a positivist' past, characterised by an overuse of survey and statistical research methods, and that it was now emerging from this past and moving towards a more pluralistic and 'anti-positivist' future. But examination of the

data from the teaching of research methods reveal that there was no so-called 'emergence' and that the very idea of a positivist past is itself a myth (this had been further supported by Platt, 1981). Neither the epistemological debates nor any growing movements within British sociology in the 1970s that may have been anti-statistical and survey methods, had any practical effect on the teaching of surveys.

It is, however, worth asking what was fuelling and sustaining this common perception that British sociology had been committed to positivist research practices from which, it was apparently, struggling to emerge in the 1970s. An article by Platt from 1981 is the only systematic critique of this argument. The article, however, is limited to an examination of the use of research methods in published sociological research. Platt shows that the amount of published sociological research using quantitative, i.e. 'positivist' methods cannot be said to have been predominant in the period 1950s-70s and that it remained consistently stable throughout this period. But what about methods teaching? Examining the question at this level allows us to distinguish between the effects that the teaching of statistical techniques, on the one the hand, and the teaching of survey method, on the other, may have had on supporting and sustaining the myth of a positivist past and anti-positivist present in the 1970s.

First, the above analysis on the teaching of methods at the LSE in the 1940s and 1950s showed that the teaching of statistical methods was done at an elementary level and limited almost entirely to the teaching of surveys and sampling. Whatever in the 1970s was fuelling British sociologists' beliefs about a 'positivist past', it cannot have been related to the teaching of statistics *per se*. The analysis of Peel and Wakeford further reveals little or no change in the proportion of universities teaching statistics at a practical and more advanced level in 1967 compared to 1978 (Appendix I, Table 23). So British sociology had never been as statistically obsessed as adversaries of statistical sociology were arguing in the 1970s; and neither has it been as deprived of statistics teaching from the 1960s onwards, as supporters of statistical sociology have argued. If there were problems with the teaching of statistics in sociology, these problems were not due to the *absence* of statistics from the sociology curriculum.

We can also see that the misconception about positivism could not have been fuelled by an 'obsession with experiments', either – experiments remained consistently the least popular methodological technique in the sociological curriculum and experienced negligible rise – from 36% in 1967 to 39.6% in 1978 (Appendix I, Table 23).

If the misconception was fuelled at all by anything that was happening in the classroom, then it is most reasonable to assume that it was fuelled by the all-pervasive presence of surveys teaching in the methods curriculum, both before 1967 and in the period 1967-1978. This makes it easier to understand why there was a mismatch between what was commonly believed about the relationship between quantitative methods and sociology among a large proportion of sociologists and what was said about this relationship in the official reports on the state of social science (see above) – while the first group could believe that British sociology was 'positivist' because of the impression created by the wide-spread teaching of surveys; the second group, the writers of the reports, supported by a small group of sociologists interested in the teaching of statistics and statistical analysis, have repeatedly insisted that British sociology has not been sufficiently empirical and statistical, despite the strong presence of survey methods teaching.

Unfortunately, the data do not allow us to reliably compare the level of survey teaching between universities and so it is difficult to judge how much of the teaching of surveys resulted in acquisition of practical empirical skills for conducting a survey. We could speculate that, since statistics is the main methodological technique used in the preparation and analysis of survey data, those universities, which taught practical and more sophisticated statistics, would also engage more closely with surveys. However, since conducting a survey is more than doing statistics; and doing statistics is more than conducting a survey, it is not possible to go beyond mere speculation.

Another important question concerns the teaching of qualitative methods. The data in Appendix I, Table 23 show a marked rise in the teaching of 'qualitative' methods. This was part and parcel of the establishment of new sociology departments and the growth of old sociology departments within universities that took place in the 60s. In the first part of the analysis, we saw that the number of sociology methods courses increased between 1967 and 1978 but that this was not

accompanied by any proportional increase in the rise of courses containing an element of statistical teaching. It was suggested that this was due to a rise in the teaching of methodological techniques, other than statistics, and we can see now that there is sound evidence to support this suggestion. It could be argued, therefore, that the rise of 'anti-positivist', and other alternative movements in the 1970s had at least one substantial effect – to increase the presence of 'qualitative' methods teaching in the sociology curriculum. However, two considerations need to be addressed before reaching any such conclusion.

First, 'qualitative' methods were *not* a small minority in 1967 – they were well represented in the curriculum and their rise was not accompanied by a decrease in the teaching of statistics and surveys. As already pointed out, between the late 1940s and early 1960s the survey method was not only the most predominant technique taught in sociology methods courses; it was the *only* one. But the Peel data suggest that by the late 1960s, this had already changed – university departments were *already* teaching a variety of research methods and that by 1967 there was *already* an existing and consistent trend towards a gradual increase in the teaching of qualitative methods. It seems likely that this trend emerged and gained momentum before 1967 and would have continued without the support of the 'anti-positivism' movement. This is not to say that the 'anti-positivism' movement did not play any role in helping to support this increase, possibly by providing a theoretical and epistemological justification for it. But it was neither the instigator nor the motive force that sociologists believed it to be.

The increase in the teaching of qualitative methods can also partly be explained by a cohort effect. As the late 60s and 70s were a period of consolidation after rapid expansion, it is reasonable to expect that more sociology graduates became available to work as university teachers in sociology, unlike earlier cohorts of sociology teachers, who were graduates of a variety of other subjects. These new cohorts would already have received some introduction to a variety of 'qualitative' methods in the late 60s and early 70s and therefore would be able to teach them at undergraduate level during the 70s. A greater diversity in the make-up of sociology graduates helps to explain the expansion in the amount and variety of methods courses that we observe in the 1978 syllabuses.

By far the most remarkable increase can be observed in the number of universities which taught a philosophy of science/philosophy of methods component – there was a nearly three-fold increase in their proportion, from 28% in 1967 to 77% in 1978; and in the number of universities which discussed 'positivism' as part of the curriculum – their proportion rose from just 4% to 35.6%. It is here, and only here, that the 'anti-positivism' movement may be said to have had an effect on what was going in methods teaching in sociology between 1967 and 1978 – they added a philosophical tinge to the teaching of methods in that period and, to an extent, influenced the type of philosophical issues, such as 'positivism' that were being discussed. Overall, however, the epistemological debates and new methodological fashions that emerged in the 1970s had little immediate effect on what methods were taught in sociology. Essentially, the so-called 'paradigm wars' (cf. Oakley, 2000) do not reflect what was going on in practice in sociology teaching.

The Teaching of Statistics in Sociology First Degrees, 1967-1978

About half of universities in 1967 and 1978 offered methods courses which contained an element of advanced or practical statistical training; this was almost always complemented by a list of recommended textbooks to assist the learning of practical statistical skills (compare the columns 'Advanced Statistical Training' and 'Practical Textbooks' in Appendix I, Table 24).

By far the greatest change in the teaching of statistics between 1967-78 was the spread of the use of computing facilities. In 1967, only about 9% of universities which taught statistical methods used computers; in 1979, this rose to around 40% (Appendix I, Table 24). But the significance of this result lies not so much in that it shows a rise in computer use but in the fact that while at the same time *computer use rose, the number of universities teaching advanced statistics remained, proportionately, about the same.* This suggests that computing at this time, at least when considered on its own, did not play a decisive role on whether and what level of statistics was being taught in sociology methods courses. Although it is reasonable to assume that computing offered an opportunity to make the statistical material

easier to comprehend and practise, it is more likely that only those universities which were already committed to teaching a more advanced level of statistics in 1967, took advantage of the newly developed computing facilities. The better availability of computers itself could not have been a powerful enough factor to convince universities teaching little or no statistics in sociology methods courses to take up statistics. And, of course, it should be borne in mind, that the 1970s was a time when computing itself was a skill that had to be learned on top of any other skills included in the curriculum. If computing made the acquisition and practising of statistical skills easier, it did so only for those staff and students who already possessed computing skills or who were in a position to learn such skills.

A revealing report published in 1970 by the Computer Board for Universities and Research Councils suggests that few social scientists were eager or even prepared to learn computing skills. Computing was still being seen as 'an esoteric or specialised activity', not as 'a versatile tool useful in any work with a factual or intellectual content' (Pierce quoted in Computer Board for Universities and Research Councils, 1970: 5). With specific reference to social science and art students, the report emphasised that that by far the biggest problem with teaching computing to social science students was *attitude*:

> To show them how a computer can be made to do things which they do not see to be valuable is of little use […] they do not see the reason for studying statistics or computing, and many of them would prefer not to know about these topics! Strong support from the staff of the Department covering their main interest in essential (Computer Board for Universities and Research Councils, 1970: 17-18).

The teaching of statistics, or lack of it, in sociology is, it seems, much more deeply influenced by factors other than technical skills and computing facilities. Engagement with statistics appears to depend, above all, on the presence or absence of a general disposition to teach and learn statistics, nurtured by the attitude of sociology teachers themselves. If sociology's engagement with statistical methods in this country changed during the post-war period, this did not happen in the decade between the late 1960s and late 1970s. The level of sophistication and rates of statistics teaching in this decade remained largely uninfluenced either by hostile

epistemological debates or by the spread of computing, suggesting the existence of much more deep-rooted and shiftless attitudes than has hitherto been recognised.

The Teaching of Methods in Sociology First Degrees in the UK, 1969-1979 according to Which University/Which Degree

Although the Peel and Wakeford syllabus collections are the major and most detailed sources of primary information on trends in the teaching of quantitative methods, slight modification of our understanding of these trends is necessary in the light of the findings from an analysis of the guide *Which University/ Which Degree*. The aim of the analysis of the guide *Which University/Which Degree* is to examine how many universities/HE institutions in the UK offered *at least one* sociology degree that contained a statistical course/course element and/or a methods course/course element in the period 1969-1979. The following analysis begins at 1969, because earlier issues of *Which University* do not contain details on sociology degree syllabuses. This is also a convenient year to start with because it means a comparison can be made with results from the analysis of the Peel data. Also for convenience, the analysis ends in 1979, when Wakeford's collection of syllabuses was published. There are differences between the data from *Which University/Degree* on the one hand; and from Peel and Wakeford, on the other (most notably in the total number of universities and HE institutions); but cautionary comparison will, at least to some extent, allow us to double check and verify our conclusions.

Which University? was first published in 1963. In 1977 it was renamed *Which Degree?* but with no changes to the content or format. Its purpose was to offer a comprehensive guide to all full-time and sandwich first degree courses in the UK; it was a popular guide with students considering applying for university who wanted to know what degrees were available and what they could expect to study in these degrees.

The guide contains a short description of degree subjects, followed by a list of the various degrees in this subject in different universities and HE institutions across

the UK. A typical description of the degree includes: the university/HE institution name; type of degree; and breakdown of the degree structure into years or stages. This is followed by a brief summary of the syllabus of the degree *as a whole* (i.e. not broken down into syllabuses for separate courses), most often in the form of key words.

Which University/ Which Degree lists *all* types of sociology degrees available in a particular university or HE institution; so, under each university/HE institution there are usually a couple of entries (e.g. BSc, BA, MA degrees or joint degrees with other subjects; or degrees in which sociology is a special subject). The unit of analysis here, however, is universities, not sociology degrees. Therefore, the analysis does not include the actual degrees that contained statistics course/course element or methods course/course element, or both. Statistics and/or methods have been marked as present at a university, as long as *at least one* sociology degree in that particular university contained such courses/course elements.

I have chosen to focus on universities, as opposed to sociology degrees for a number of reasons. Firstly, there are significant differences between the degree types in which sociology is present; the great variety of degree types and the lack of detailed information in the guide make it very difficult to compare degrees. Secondly, it is not my aim to distinguish between universities on the basis of how much statistics and/or methods were offered as part of a sociology degree (this would not be possible in any case since the description in the guides is too short). What I really want to find out is how many universities offered even just the prospect of studying statistics/methods in their sociology degrees.

Since the degree summaries describe a degree as whole, it is not possible to distinguish between courses and course elements within a degree syllabus – a key word or phrase mentioning "statistics" or "methods" in a degree summary may refer to a whole course in one degree, but only to an element within a course in another degree. At best, therefore, the summaries are an indicator of a minimum engagement with statistics or methods. There is no information as to the quality, type, content or length of either statistics or methods courses/course elements; therefore nothing can be said about their role in the sociology courses/degrees; about how well they are incorporated and what proportion of the degree is devoted to statistics or methods.

However, *Which University/ Which Degree* remains a useful and important source of data since it collected syllabuses from universities and HE institutions consistently and systematically and presented them in a uniform manner. In fact, *Which University/ Which Degree* is the only source of this type of information that could be used for a systematic analysis, year by year, of the contents of sociology degrees across the UK during the 1960s. Contacting each and every university and HE institution in the UK and asking them to provide syllabuses from that period may have provided me with more detailed/reliable information, but it would have been extremely time consuming, with no guarantee of success.

The description contained in the degree summaries allows us to distinguish between statistical courses/statistical course elements and methods courses/ course elements since very often statistics and methods are listed as separate key words/phrases. My experience in the analysis of course syllabuses from the period also suggests that it was not uncommon to treat statistics as a category separate from methods; plus in the period under investigation, there was no sharp distinction between the various qualitative methods, which were usually listed as one category. Of course, it is possible that the universities which offered only 'methods' may have failed to specify statistics as a separate category and, unfortunately, I have no way of finding out how often this might have been the case.

The presence/absence of statistics and methods courses/course element in universities HE institutions has been marked in Appendix I, Table 25. Given that this analysis is based on rather limited data, it would be best to think of these figures as an indication of the minimum statistics/method teaching, i.e. not as an indication that the universities that offered sociology degrees containing them actually produced statistically or methodologically capable students.

Tables 25 and 26 in Appendix I show that a fair amount of universities in the period 1969-1979 offered at least one sociology degree which contained a statistics course/statistics course element; we also see a slight decrease in the amount of universities which offered at least one sociology degree that contained statistics course/course element in 1969 as compared to 1979, from ca. 60% to ca. 50%. These proportions are smaller compared to the results from Peel (1968) and Wakeford (1979) which showed that in 1967 84% of universities taught statistics/survey

methods, while in 1978 this number had diminished to 81.3% (Appendix I, Table 27). How do we explain the difference in proportions and the larger decrease in the numbers of universities teaching statistics/surveys shown in *Which University/ Which Degree*?

Which University/ Which Degree collected data from more universities than Peel and Wakeford. As I concluded in the analysis of the Peel and Wakeford data above, if there was a decrease, this was more likely due to the expansion of sociology in new universities where the focus was on the teaching of other methods, rather than because universities where statistics and/or surveys were taught suddenly stopped teaching these methods. That the expansion of sociology in existing universities and in new universities created more space for the teaching of non-statistical and/or non-survey methods is also evident from the overall rise in methods teaching shown in the *Which University/ Which Degree* (from ca. 70% to ca. 83%); and from the decrease in the amount of universities offering *only* a statistics course/course element. If we allow for the fact that with the rise of methods teaching during the 70s many more universities may have included statistics teaching within methods teaching (thus failing to put 'statistics' or 'survey' as keywords in the description and only putting 'methods') then we can reasonably conclude that the decrease in the teaching of statistics/surveys was a bit smaller than 10% (the difference between Which university (1969) and Which degree (1979)) and a bit larger than 3% (the difference between Peel (1968) and Wakeford (1979); which is far from a dramatic drop.

There are only three universities that offered a joint degree Sociology and statistics/quantitative methods in this period. In the background of many other types of joint degrees, the combination of sociology and statistics appears to be rather rare, which, apart from anything else, is an indication of a low demand on the part of student applicants, and thereby of a rather weak link between both subjects.

Methods Teaching in Sociology, 1967-1978: Summary

The analysis of Peel's and Wakeford's syllabus collections and the material in *Which University/ Which* Degree shows that by 1978, the teaching of methods had become an essential part of the undergraduate sociology degree. However, it has also shown that as new sociology degrees emerged and the old ones expanded, methods teaching developed in a direction which ascribed increasing importance to non-quantitative and non-survey methods, including 'qualitative methods', teaching of philosophy of methodology and discussions on 'positivism' in methodology.

Although the provision of quantitative methods teaching in sociology first degrees was consistently present and stable in the late 1960s and in the 1970s, the mere presence of some statistics in sociology degrees does not necessarily mean that UK universities were producing statistically literate sociologists. Failure to make this distinction, and also failure to distinguish between elementary survey teaching and the teaching of statistical methods and thinking per se, helped to fuel a powerful, but as I have shown, misguided, conception in the late 1960s-1970s that British sociology had been, and was continuing to be, obsessed with empiricist or 'positivist' types of teaching and enquiry. The analysis of Peel's and Wakeford's collections, shows that this was illusory – the level of sophistication of most statistics courses was elementary and with a subject matter like statistics, this is crucial – statistics is probably one of the hardest and most demanding[39] elements that could possibly feature in a sociology degree; a little exposure to statistics cannot easily be seen as being of much use to the student; no more than merely letting them know about the existence of statistical methods. The understanding of sociology as a subject intended to provide a general education, discussed at the beginning of this chapter, is, therefore, clearly manifested in the teaching of methods.

US Views on the Teaching of Sociology in British Universities

[39] Methods skills are of course learnt best through practice; what I mean by saying that statistical methods are 'one of the hardest' elements in a sociology degree, harder than qualitative methods, is that, allowing for some variation on individual level, on average students would need a lot of extra preliminary knowledge about mathematical statistics and computing software even *before* they can begin to practise statistical data collection and analysis; this preparatory stage in qualitative methods training is much simpler and undemanding.

The analysis of the teaching of sociology at the LSE, as well as the analysis of the methods teaching in sociology based on the material from Peel's and Wakeford's syllabus collections and *Which University/Which Degree,* strongly support the argument that the teaching of quantitative methods in sociology was generally basic and could not have produced cohorts of statistically literate sociology students. A question then inevitably arises as to how the teaching of sociology was seen by sociologists from other countries, such as the USA, where sociology had developed much earlier and much more extensively than in Britain[40]. In the remaining part of this chapter, I examine American sociologists' views on the teaching of sociology in Britain. The aim is not to portray such views as commendable but, just as in the previous discussion of the official reports whose recommendations were consistently at odds with mainstream sociology, this discussion should serve to show that there existed an alternative and legitimate viewpoint on the relationship between statistics and sociology in Britain.

The sociological background of the American sociologists who visited Britain and commented on the state of sociology during the twentieth century differed greatly from that of their British colleagues. A strong link between American sociology and statistics was forged during the 'formative period' in the history of American sociology between 1890-1940 (Camic, 1995: 1005). Clear understanding that collaboration between the two subjects is not only necessary but vital is evident in a number of the most prominent American sociologists from that period, such as Richmond Mayo-Smith, Franklin H. Giddings and William F. Ogburn (cf. Mayo-Smith, 1902; Ogburn, 1927; and Goldthorpe, 2007). More importantly, this idea got support and was realised on an institutional level – sociology departments such as Columbia but also Chicago (see Bulmer, 1984) became well known for advancing quantitative sociology (for an extensive account on the early incorporation of statistical methods in the social sciences, see Walker, 1929). It has been argued that the expansion of social science in American universities at the beginning of the twentieth century actually stimulated the incorporation of quantitative methods across social science subjects such as sociology, psychology and economics by

[40] I acknowledge that understanding the significance of the British situation in comparative perspective would require examination of the views of sociologists from other countries, too; but this should remain a topic for further enquiry.

creating a competitive environment in which these subjects had to quickly and effectively prove their scientific status and claims; and indeed, the evidence is clear that social science departments invested a lot of time and effort into incorporating the latest developments in statistics (ironically, almost all, if not all of them were being invented in Britain, not in the US).

The 'statistical turn' in American social science has been interpreted as having been a result of a 'boundary work' taking place within social science departments; but also boundary work between social science departments and departments of natural science (cf. Camic and Xie, 1994; Camic, 1995). Statistics offered a solution to 'the newcomer's dilemma': faced with the question of how much to conform and how much to differentiate, social science subjects could rely on statistics as a way of conforming to the established understanding of what it was to be scientific; but also use different statistical techniques to develop innovative and attractive approaches that would make them exciting and attractive. But although there were in the USA favourable conditions for the development and sustaining of a tradition of quantitative sociology, this resulted in a series of complications and problematic situations that persist in American sociology to this day – Goldthorpe (2007) points out that although American sociology has had the willingness and resources to engage in serious quantitative work, an effective and fruitful merging between sociological theory and statistical methods remained unaccomplished; throughout the twentieth century there has been a 'working' but nonetheless 'loose' consensus about the factors responsible ('failure of interests to converge; embracing of critical positivism, psychologism etc. – for a full explanation, see Goldthorpe, 2007). Others, such as Bannister (1978), have developed more radical arguments, pointing out the role of early incorporation of statistics in American sociology as a source of American sociology's 'scientism'; while Turner (2014) has associated the quantitative tradition with the emergence of what is perceived as 'elite sociology' in the American context, bringing further division and misunderstandings.

It is clear from what has been written by a variety of scholars about the relationship between American sociology and statistics that although it would be fair to say that this relationship has thrived in particular circles, it was a product of distinctive intellectual and institutional conditions and that it has also brought about

distinctive challenges. But possessing general knowledge about the development of American sociology can help in our understanding of the British situation. For instance, only after we observe how the incorporation of statistics into American social science was instrumental in solving a 'newcomer's dilemma' are we alerted to the fact that in the academic situation in Britain in the post-war period, sociology was a *newcomer without a dilemma* – the subject became popular and demand was rising unconditionally; British sociology did not have to fight the fight that scholars have argued took place in the USA. Of course, this is not to say that had sociology in Britain been faced with a similar 'newcomer's dilemma' this would have automatically meant that British sociology would have been more receptive to quantification.

Knowing of the American experience is also valuable because it shows that a quantitative tradition was a genuine option for sociology; and that it was an option even if intellectual resources had to be borrowed from elsewhere, as was the case with the new English statistics, developed towards the end of the nineteenth and the beginning of the twentieth centuries which the Americans borrowed and began using almost immediately. What this tells us is that the mere existence of a well-developed tradition of statistical enquiry, like the one we see in Britain in the nineteenth century, is not enough on its own to stimulate the incorporation of this tradition into mainstream or general or academic sociology. A more powerful, and indeed vital, factor was *receptiveness*; including, at the very least, an open attitude to statistical knowledge and statistical methods – the British had the home-grown statistical knowledge, but did not have the receptiveness. In the USA, it was *vice versa*.

Last, but not least, this comparative overview of the American background can help explain why the American observers had the views they had with regard to British sociology (see below). But it can also show that their evaluations, albeit using the American situation as their measuring stick, were driven by legitimate concerns – they did not just see British sociology as odd because it was unlike American sociology with respect to quantification; they saw British sociology as odd because it had rejected a *legitimate* path that they themselves had found fruitful.

American sociologists first commented on the state of sociology in Britain as early as 1901. At the Paris Exhibition that year, Lester Ward, one of the founding

fathers of American sociology observed that 'the sociological movement' was burgeoning in all western countries but that it was 'perhaps least in England' (Ward, 1901:1454). 'Aversion to theory', rejection of the thought of those who had become prominent but struggled to facilitate a sociological movement, like Herbert Spencer; the fact that enormous responsibility in investigating and organising social life was in the hands of the British government and municipal authority, were all cited as possible reasons as to why Britain was lagging behind other countries. This list of reasons which, among other things, neglected the British tradition of empirical social enquiry and the opportunities that it held for sociology, shows a parallel way of thinking between Ward and the future founders of the first British Sociological Society; but while in America, at the time Ward was commenting on Britain, there were sociologists working towards a more statistical sociology and a more sociological statistics (Mayo-Smith, Giddings), in Britain there were none.

Some years later, Palmer (1927: 360) observed that regardless of the efforts of the Sociological Society, this situation had changed very little, with the London sociology department, the rather dormant LePlay House and the so-called 'Edinburgh school' comprising all the sociology that was being done in Britain; while Barnes (1961: 794)[41] commented that the two professorships established at the beginning of the century were 'a fortunate academic accident' in spite of which sociology was still 'almost as rare as gold in sea water'. Harper (1933: 335) described its condition as 'moribund' and 'undeveloped'.

As time passed, it seemed that the list of reasons why British sociology was lagging behind was growing but no satisfactory explanation was forthcoming. Barnes, for instance, argued that the root of the problem was in the British higher education system. With the exception of London, Barnes argued, British education was 'still primarily medieval' and 'concerned chiefly with the classics, dialectic and metaphysics' (Barnes, 1927: 46). It appeared that a subject like sociology had no place in universities whose chief goal was 'to educate gentlemen'; to train men 'how to argue with charm and lofty detachment rather than how to investigate with

[41] Barnes (1961) is an almost exact copy of his 1927 article with some further but negligible additions. It is unclear why it has been decided to reprint this article without notifying the reader that the material in it dates back to the 1920s. Americans reading in 1961 would have been misled by this account which gives the impression that the state of sociology in 1961 was the same as in 1927.

precision'; the whole process of British education was summed up as 'a dignified and seductive flight from reality' (Barnes, 1927: 46). Examining the development of sociology in Britain in greater detail reveals a situation more complex than this, although, just as we would have expected if we followed Barnes, once the higher education system was reformed, there was suddenly plenty of room for sociology. But, as I have shown, as time went on this created new problems and brought old problems into prominence. America began to pay less attention to the late arrival of sociology in Britain and more to what had become of British sociology once it arrived.

Harper suggested that the strategy embraced by Hobhouse and Ginsberg was the crucial factor – the 'catholicity' and 'comprehensiveness' of their approach to sociology had led them to minimise rather than accentuate the difference between sociology and other social sciences and to assume that sociology's primary functions were 'philosophical and integrative (Harper, 1933: 339). It was because of this that sociology had failed to expand academically and gain for itself 'any wide acceptance as a distinct scientific technique' (Harper, 1933: 339). But in addition to this, Harper noticed that part of the problem had to do with the lack of integration of the empirical tradition of social enquiry that had developed in Britain during the nineteenth century and that had continued to develop quite separately from sociology. A lot of what in Britain was called 'social science' or 'social study', said Harper, 'might well pass as sociology in the US', bringing attention to this rather odd split that would continue to surprise Americans commenting on British sociology. It is ironic, but also illuminating, that British sociologists themselves rarely pondered on this question. If this was done at all during the twentieth century, it was always by a minority of sociologists with a specialist interest in methods such as in the 1981 BSA conference (cf. Burgess and Bulmer, 1981); or in official reports. A further confirmation of this is, of course, the fact that the present thesis is the first time this question has been examined in detail and in depth.

Later American commentaries elaborated upon Harper and Barnes. Burns (1951) was not as radical as Barnes in his comments but presented the peculiarities of the British education system as a major factor in the backwardness of academic social science, saying that because the older universities had always insisted that

their function was ‘to train the powers of the mind, not to give much positive or any professional knowledge’, social sciences faced twice as many difficulties as other subjects – not only had they to prove that they are ‘not professional’ but they also had to show that ‘as a training of the mind, they were equal to classics and mathematics’ (Burns, 1951: 232). The analysis in previous chapters has shown that once an expansion of the education system was under way in the post-war period, this is exactly the path that British sociology chose to take. But Burns’ account is even more valuable because it identifies a deeper and, as I have shown, a more pervasive element determining the character of British sociology – its attitude to research methods training. Such training, Burns noted, ‘had developed but slowly in Great Britain’ and ‘was regarded as the pastime of the enthusiastic individual’ (Burns, 1951: 245). This was also picked up by Selvin (1965) who related such general attitudes towards research training to the more striking issue of the non-quantitative character of British sociology. Selvin was:

> surprised to find out that the British not only lag behind America in their teaching of methodology and in their training for research but that they are also far behind many European countries (Selvin, 1965: 73).

The backwardness that has persisted in Britain was ‘all the more surprising’ *despite* ‘the long British tradition of empirical social research (Graunt, Sinclair, Farr, Booth, Rowntree)’ and ‘the pre-eminent place that Britain occupies in the development of modern statistics as a set of tools for research (Pearson, Yule, Edgeworth)’ (Selvin, 1965: 92). This was the first time that the development of sociology in Britain was questioned not so much on its ‘late’ development, but because of its failure to utilise the advances made by the empirical tradition of social enquiry that preceded it. British sociology ‘may well ponder the reasons for its failure to exploit its past opportunities’ but also ‘the changes that might be made in the future to bring it in step with sociology elsewhere’ (Selvin, 1965: 92). British sociology may have differed from the sociology in other countries in the sense that it arrived relatively late; but what was becoming increasingly more obvious and urgent to observers from other countries was its odd attitude towards quantification. The

American experience was not there to be emulated but to help put things into perspective – an important perspective, as Selvin's account makes clear.

The Teaching of Sociology to Undergraduate Students in Britain, 1904-1979: Summary

This chapter analysed the practical consequences of British attitudes and arguments about the nature of sociology and its methods for the teaching of sociology. These attitudes, and the arguments they gave rise to, dominated the discussions within sociology throughout the twentieth century. The chapter showed that just as statistics was present in these discussions but was met with indifference, or disapproval or sometimes even outright hostility, so statistics was also present in the sociology curricula, especially in the early post-war period but was far from adequate to produce cohorts of statistically literate sociologists. It is in the teaching practices that the impact of the views and attitudes of sociologists is most strongly felt – with remarkable consistency throughout the twentieth century, British sociology has been producing successive generations of sociologists who have little familiarity with quantitative methods, let alone enthusiasm. The lack of incorporation of statistical methods and thinking into sociology has been affected not only by explicit action against quantification taken in vital moments in the history of British sociology (for instance when contents of curricula was being decided during the expansion); but also implicit acceptance on the part of students and younger researchers that the sociology they have been taught is of the right kind, perhaps the only kind. Not having been taught statistics at all, or only superficially, or even to have had statistics disparaged by their mentors is bound to have affected their own work and teaching and reinforced the idea in their own minds that sociology is, indeed, a non-statistical subject. The next chapter turns to the period of the 1970s and early 1980s to show that although this was 'a difficult decade' for British sociology; 'something of a crisis of confidence' (Review Committee on Sociology, 1989: 12) the change in sociology's public status and self-perception had little effect on the way sociology perceived the role of quantification and statistical training.

Chapter Thirteen

Positivism and Statistics in Sociology in Britain, 1970-1990

The Disputes about Positivism[42] in British Sociology – Overview of the Context

The 1970s was a difficult period in the history of British sociology and has been described as 'something of a crisis of confidence' (Review Committee on Sociology, 1989: 12). Pressure was mounting on all sides – there was pressure from outside due to the 'unrealistically high expectations that were imposed on the subject' during the expansion period; there was pressure from the SSRC striving to get sociology to engage in more 'fundable research'; and there was intellectual pressure from rising groups, such as feminism, Marxism and ethnomethodology, 'undermining empirical research and leading to the disputes on positivism' (Review Committee on Sociology, 1989: 12). The younger generation, it was argued, 'talked phenomenology and Marxism and their students were switched on to little else but theory' (Rex, 1978b: 295). But the new alternatives failed to provide instant solutions to British sociology's intellectual problems: the dominant influence of Marxism, phenomenology, ethnomethodology and the like was understood to be hardly due to 'the internal validity' of these new approaches; it was rather due to the fact that they fitted well with the new social movements (Rex, 1978b: 295). Thus it was not only the students' choice to study sociology that had a religious tinge to it, as described in Chapter Eleven; it was also sociologists' choices on what and how to research, leaving them, in Rex's words, with 'dogmas and cults, even with intellectual wars of religion' (Rex, 1978b: 295).

Regardless of all this, it was still commonly believed that the empirical tradition of social enquiry and also the functionalist movement of the 1950s were 'the enemy', and that the 'best way to make our point against the traditions was to fight for a more theoretical approach' (Rex, 1978b: 295). The presence and increasing influence of new philosophical alternatives that emphasised theoretical

[42] I have put 'positivism' and 'positivist' in inverted commas only in those cases in which I refer to 'positivism' and 'positivist' *as terms*. In all other cases, I use these words (and derivatives of them) without inverted commas.

work at the expense of empirical enquiry resulted in a situation in which critiques of empirical and quantitative research became more vocal and direct. Attitudes towards quantification changed little, but they now existed in a more polarised climate and those who subscribed to the empirical tradition were labelled 'reactionists' or 'positivists' and were effectively marginalised. Clearly sociology was about something else that was beyond empirical enquiries:

> How many now dare own up to being an empiricist in the tradition of Booth, Rowntree and the Webbs? In general, sociologists today believe that gathering information, in the hope that this will make the world a better place, is an activity more appropriately performed by civil servants and social administrators (Pahl, 1974: 504).

The identification of quantitative studies almost exclusively with surveys remained consistent – it was commonly argued that by survey research one usually 'finds out at considerable expense what can be discovered by looking things up in a book' (Unknown Author, 1971: 833). In addition to the perceived inability of survey research to produce new and surprising knowledge, the validity of the knowledge coming out of survey research continued to be seen as highly questionable, because of its dependency entirely on 'the precision of the question' and 'the honesty' of the answers (Unknown Author, 1971: 833). Like earlier critiques which were discussed in previous chapters, these too failed to explain how, if at all, any of the other methods available to sociology were different or better in this or other respects. How and why sociologists thought that they could have special access to the social and internal world of people in society, thereby overcoming the problems of having to ask people questions and rely on the honesty of their responses, was never properly addressed.

Although it has been a running theme of the third part of this thesis that discussions and decisions regarding the methodological development of sociology were rarely based on practical concerns and rational arguments (of the sort that all methods are equal and that the choice of research method necessarily depends on the research question), in this last chapter this trend becomes more pronounced; it was politics and preferential attitudes that determined the views and treatment of research

methods. This is nowhere as obvious as in the disputes about positivism that took place in British sociology in the 70s and 80s.

In the following sections, I examine these disputes, paying special attention to *how* the disputes played out – who the involved parties were and what they were trying to achieve – and less attention to the strength of the arguments used. Unlike existing accounts of these disputes, which focus more on which side appear to have presented a stronger case, it is my aim to show here that the way in which these disputes happened and the *kind* of influence they had historically, particularly with regard to the relationship between sociology and statistics, is more important than who 'won' them.

What is Positivism in Sociology?

The disputes about positivism in British sociology took place in a context in which there was much confusion as to what positivism as a way of thinking about sociology and practising it actually meant. One of the clearest explanations comes from Kolakowski (1972) according to whom a positivist philosophical outlook was an outlook that fulfilled three criteria: phenomenalism and nominalism which together imply that all knowledge derives from experience and not from a deeper reality (such reality was understood to be impossible); and separation of facts from values. In British sociology in the 1970s, these philosophical principles have been understood as a belief that 'the goal of sociological analysis can and must be to formulate laws'; that 'methodological procedures of natural science may be directly adapted to sociology' and that the findings of sociological research can be value-free (for a fuller summary, cf. also Giddens, 1974: 3-4).

It is important to note that, historically, statistics had not been understood as a positivist method. Auguste Comte, who first formulated and explained what positivism is, argued that a positive sociology is a sociology aimed at the formulation of laws. But according to Comte, statistical enquiries had *no place* in positive sociology because statistics was based on and could only reveal *uncertainty*, leading to tentative conclusions rather than universal laws. Positivism as developed in the

philosophical Vienna circle at the beginning of the twentieth century was more strongly related to quantification but, overall, any association between 'quantitative' or 'statistical' and 'positivist' that we might find in British sociology in 70s and 80s would be a result of particular factors in the post-war development of the subject and not necessarily a result of the historical meaning of the term 'positivism' as it was used in the nineteenth and early twentieth centuries.

Defining positivism in abstract terms and on paper was easier than describing it with reference to real examples from sociological teaching and research. There was great difficulty in trying to find somebody in post-war British sociology who was actually doing sociological work in line with the positivist criteria described above. Some sociologists were being *described by others* as positivist usually without concrete examples to support such allegations; but positivism was 'almost never a self-applied label' (Marsh, 1982: 48). In addition, Marsh did not believe 'that the story would contain much reference to the printed word'; rather it seemed to be 'part of the conscience collective of sociologists' (Marsh, 1982: 48). Giddens observed that positivism 'has been used so broadly and vaguely as a weapon of critical attack […] that it has lost any claim to an accepted and standard meaning' (Giddens, 1974: 2). Phillips followed, arguing that 'while the word is full of sound and fury, it signifies nothing' (Phillips quoted in Oakley, 2000: 30). And a study by Platt showed that deeply entrenched perceptions, common within British sociology in the late 70s, that a 'positivist' tradition had existed in British sociology in the earlier post-war period, were largely unfounded (Platt, 1981). These examples suggest that the overall confusion arose due to a gap between certain *beliefs* about the existence of positivist sociological research and the actual existence of positivist practice, and in mistaking one for the other.

The problems with using terms such as 'positivist' and 'positivism' are further explained by Bryman who observed that the disputes were characterised by a 'tendency for philosophical issues and technical issues to be treated simultaneously and occasionally to be confused' (Bryman, 1984: 75). This meant that if the arguments in the disputes are taken to the level of the teaching of methods and the use of methods in research, then it would quickly become apparent that it was difficult, and in most cases unjustified, to establish a clear symmetry between

epistemological positions, which were the focus of the debates, and research and teaching practices.

More recent comments on the positivist disputes do not differ much from these contemporary evaluations. Goldthorpe remembers that he has 'never been able to get from people who talk about positivist sociology any coherent definition of what they mean'; that they have been 'quite incapable of explaining what it means' (Author's Interview with John H. Goldthorpe, 2017). One could guess that in some cases the anti-positivist attacks were aimed at quantitative sociology, and so 'positivist' would simply mean 'quantitative'; other times one was under the impression that the attacks were trying to assert the importance of a sociology that has 'some humanistic meaning and significance'; but Goldthorpe, at least, did not see how this should have led to a rejection of quantitative sociology which could also have 'some humanistic concerns' (Author's Interview with John H. Goldthorpe, 2017). And, in Moore's view, 'the people using the word "positivism" were misusing it and […] did not understand what positivism was' (Author's Interview with Robert Moore, 2017).

Positivism, therefore, may have begun as an important and relatively clearly defined *philosophical* debate about the unification of all science, as in the Vienna circle; and also as a debate about the source and production of knowledge, as, for instance, in the debates between Popper, Habermas, Adorno, Gellner etc. But, when it came to sociology in Britain in the 70s, the *debates* turned into *disputes* which were not at all aimed at bringing about more clarity, unity and improvement of methodological competence in the next generation of sociologists.

Evidence for this comes, for instance, from the analysis of the Peel (1968) and Wakeford (1979), syllabus collections and of the material in the guide *Which University/Which Degree*, which was presented in the previous chapter. The analysis showed clearly a remarkable continuity through the 60s and 70s in what sociology students were being taught in methods courses. It showed that the number of quantitative methods courses remained the same; while a rise in qualitative methods teaching was apparent before the advent of the positivist disputes. All this challenges the claim that the rise of feminist, symbolic interactionist and ethnomethodological movements, that together were said to have amounted to an anti-positivist movement, transformed the character of the *whole* of British sociology. The fact is that the

disputes about positivism had little real impact and changed little *in practice* for sociology students in this period, except, perhaps, for infiltrating methods courses with discussions about positivism.

If there was overwhelming lack of clarity about the meaning of the term 'positivism'; if there was very little, if any, reference to real examples from research and teaching in the disputes about positivism; and if these disputes had little practical impact on what was being taught in sociology methods courses, then what sustained these disputes? Why did they become influential and how come there was so little done to disperse the confusion?

Part of the explanation lies in the function that these disputes had in the development of sociology. Positivism in British sociology in the 70s differed from other intellectual movements that had been popular previously, such as Marxism or functionalism, in that, unlike them, the terms 'positivism' and 'positivist' were used less as a description and more as an insult. The term had very quickly become 'one of opprobrium' (Giddens, 1974: 2); 'a term of abuse first and foremost' (Marsh, 1982: 48); it was 'widely used as a generalized term of abuse' (Phillips in Oakley 2000: 30); and was 'just a convenient…and slightly abusive label to hang onto people who …actually collected and analysed data' (Author's Interview with Robert Moore, 2017). The fact that the terms 'positivist' and 'positivism' were used as an insult suggests that 'anti-positivist' arguments did not actually *need* to make reference to reliable evidence about the existence of positivist sociological work. This explains partly why the disputes could spread regardless of the fact that they did not refer to real examples.

Another explanation of the spread of the disputes about positivism refers to the close links that existed between philosophical positivism (as described by Kolokowski above) and the rise of feminism and the arrival from the USA of movements such as symbolic interactionism and ethnomethodology. Oakley (2000) has argued that those who mounted anti-positivist critiques referred to three particular works on methodology for the basis of their critiques and in making alternative suggestions: Cicourel's *Method and Measurement* (1964), Glaser and Strauss' *The Discovery of Grounded Theory* (1967) and Garfinkel's *Ethnomethodology* (1967). It is widely believed that together with feminism, 'as a

social movement which infiltrated academia and also had a great deal to say about methodology' (Oakley, 2000: 32), these developments brought about a state of 'epistemological anomie' (Bell and Newby, 1977) and gave rise to 'paradigm wars' (Oakley, 2000). The ultimate aim was to make room for feminist and other non-quantitative approaches by stirring up a particular polemic within the sociological community. This polemic aimed at creating a sense of solidarity among disparate groups of not-quantitatively minded sociologists and at serving their professionalising agendas, especially the agendas of emerging feminist sociologists. Thus by the late 1960s and early 1970s, qualitative research came to be highlighted quite unambiguously as the preferred, non-positivist paradigm, with quantitative research being ear-marked as the work of the 'patriarchal devil' (Oakley, 1999: 249).

In this sense, it would appear that it is not quite so surprising that the disputes on positivism had little effect on the contents of the methods curriculum in the 70s and that they spread despite the lack of clarity about the meaning of positivism. The world of the positivist disputes and the world of methods teaching were, it would appear, two separate worlds – one was a world of polemics and epistemology, the other a world of methodological techniques; one was philosophical and abstract, the other – practical and precise; one was a world of research, the other – a world of teaching. We simply could not expect, Oakley argued, 'the paradigm argument to make much sense on a practical level' (Oakley, 2000: 42).

If the disputes about positivism were aimed at discrediting the view that sociology's ultimate aim was the formulation of general laws; and if they had little practical impact beyond creating intellectual space for feminist, ethnomethodological, and symbolic interactionist movements, were they in any way important in the relationship between sociology and statistics in Britain in this period? What impact, if any, did they have on this relationship?

The Disputes about Positivism and Their Impact on the Relationship between Sociology and Statistics in Britain

The disputes about positivism are particularly important for understanding the relationship between sociology and statistics in Britain because they turned a long-standing disagreement among British sociologists regarding the roles of theory and methods in the development and teaching of sociology into a bitter antagonism, sustained by lack of dialogue and lack of mutual understanding.

The discussion in previous chapters has shown that even before the disputes broke out in the 70s, for the majority of British sociologists, theory was a primary concern and was the highest status activity in sociology. This continued to be the case in the 70s and 80s: 'There was this sort of feeling of 'We must be theoreticians" John Wakeford remembered recently; the idea that the 'real sociologists were the theoreticians […] the people who wrote treaties without any reference to data collection' was exerting a strong influence on how methods teaching was perceived and on sustaining a divide between theory and methods, especially in teaching (Author's Interview with John Wakeford, 2017). Post-modern, interpretivist and feminist sociologists, for instance, understood theory as a self-sufficient philosophical exercise and methods, usually qualitative methods, as something that merely *demonstrates* theory. Partly as a result of this understanding, prevalent in British post-war sociology, methods remained a secondary concern. The teaching of methods, for instance, was an unpopular task usually done by somebody who was often forced to do it:

> […] very often in sociology departments the last person to get appointed got asked to teach the methods course. […] When more staff were recruited or replaced, then somebody else will get recruited to do that. […] methods teaching was not of high status [...] I remember talking to colleagues who said 'Oh, the last thing on Earth I want to do is to teach research methods' (Author's Interview with Robert Burgess, 2017).

Further evidence comes from the planning of Wakeford's (1979) and Burgess' (1979) syllabus collections to which I referred in the previous chapter. These collections were prepared for a conference on the teaching of methods that was held in Warwick in 1979. Although at first glance it might appear that the conference was a sign that increasing attention was being paid to methods and methods teaching across the sociology community, including those for whom theory was primary, Burgess' recollection of the event indicates that the conference was not very

influential beyond the small community of sociologists who were *already* interested in methods and who took part in the conference (Author's Interview with Robert Burgess, 2017). And with regard to his syllabus collection, Wakeford recalled that the initiative did not arise from greater attention being paid to methods teaching but rather:

> because we were getting contacts from people in the new universities [...] who decided to launch sociology and methods and they sort of kept sending things saying 'What's taught elsewhere?'. So I think that's the origin of that. We just put it together and then, say, if anybody wants to know what is taught elsewhere, there is this compendium (Author's Interview with John Wakeford, 2017).

There were sociologists with strong interest in the role of methods in the development and teaching of sociology but, as in previous decades, they remained a small minority. Even smaller was the group of sociologists specialising in, and advocating the use of, surveys and statistics. They saw theory differently from the majority of sociologists and similarly to the way most natural scientists see it – as a network of falsifiable generalisations. And they saw methods as serving the role not of merely demonstrating theories but of testing them; and for them quantitative methods especially were a primary concern and a means by which sociology would be able to advance in this respect.

The disputes about positivism did little to shift the balance between 'theorists' and 'quantitative sociologists'; overall, the former remained in the majority and the latter – in the minority. However, these disputes played an important role in exacerbating this divide and providing a justification for it.

Anti-positivist critiques did not target just any type of empirical sociological work – they were specifically addressed to social science and sociology that used survey and statistical methods. In these cases, confusion about the contents of the label 'positivist' did not stop some anti-positivist sociologists from applying this label to groups of researchers *by association.* Up to 1970s, statistics and surveys were unpopular, unfashionable and seen as far from being a priority; during the 1970s, they came to be seen as *inherently* positivist.

Examples of the association between positivism and survey and statistical work comes from a variety of sources. Oakley (2000) who examined the feminist anti-positivist movement in particular, argued that:

> The statistical dominance of men and the use of quantitative methods may not have been intrinsically related; but they were seen to co-exist in a (more or less) unconscious conspiratorial alliance (Oakley, 2000: 33).

Other feminists referred to the presence in quantitative research of such things as 'evidence', 'fact' and 'technical expertise' (cf. Stanley and Wise, 1983: 8) along with the notion of the existence of an objective reality in order to explain the association between positivism and quantitative research. Anti-positivist feminists objected to quantitative research (and thereby to positivism) by arguing that objectivity is:

> an excuse for a power relationship every bit as obscene as the power relationship that leads women to be sexually assaulted, murdered and otherwise treated as mere objects (Stanley and Wise, 1983: 169).

They saw:

> positivist reality as invalid – but only for us. What we mean by this is that positivist reality isn't just reality for positivists […] positivist reality is their generalized, universalized view of our realities. We object to our lived experiences being turned into generalized mush (Stanley and Wise, 1983: 111).

But who are the positivists? Whose is this reality? It appears as if the rejection of evidence and facts that the authors hold to be commendable is responsible for their own failure to support their own statements with evidence. Perhaps providing evidence for their arguments would have been against their own *beliefs*. And other critics of positivism argued that 'much of the statistical work done in social science has been based on a positivist view of science', quoting as an example Emile Durkheim's study of *Suicide* but no *contemporary* British research (Keat, 1979: 75-79).

A clear association of quantitative techniques with positivism, and therefore with anti-feminism, has been noticed even by those who would not necessarily agree that there was much sense in the claims that quantitative social research was somehow male or automatically at odds with feminist causes:

> So it came as a bit of a surprise to me to find later on that there were some women, some feminists – and therefore some men as well – arguing that quantification was an intrinsically male way of looking at the world and the reason it was astonishing was that I already knew dozens and dozens of extremely good, intellectually acute women, working in quantification, for whom it would have been deeply insulting to be told that what they were doing was intrinsically male. So, I'm afraid I had a prejudice against this particular kind of rhetoric. [...] And I've never really been able to take it seriously because I've never seen any serious development of that idea (Author's Interview with Lindsay Paterson, 2017).

> […] that was enormously influential as an idea […] But the idea that women in particular are being constrained by having fixed questions in a questionnaire, just seems to me silly. But things that were not much grander than that seemed to me to form part of what was becoming an orthodoxy […] people had developed rationales for the kind of method that was compatible with their sort of feelings (Author's Interview with Jennifer Platt, 2017).

Ironically, the apparent legitimacy of the association between quantitative methods and positivism was reinforced by the replies that quantitative sociologists gave to counteract critiques such as those raised by feminist sociologists, even though in most cases the purpose of these replies was to disprove the existence of the association altogether. A study by Platt (1981), for instance, used an analysis of the *quantitative* sociological research published in the three leading sociological journals in the early post-war period in Britain to argue that 'the existence of a clearly defined *positivist* style is highly questionable' (Platt, 1981: 76, *italics mine*). Although Platt provides a summary of the philosophical positivist principles and acknowledges that these are more relevant to an abstract discussion than to describing sociological research practices, she is nonetheless forced into the assumption that one could look for these principles in quantitative research. This is not saying that Platt herself believed this to be the case. Her article does not prove that quantitative research is not positivist; it only shows that because little quantitative work was done in Britain

in the early post-war period, therefore there was also little positivist work done. Inadvertently, Platt ends up equating the two and legitimising the assumption of an association between them.

It was Catherine Marsh's studies (1979, 1980, 1982) that aimed to prove *wrong* the association between survey and statistical work and positivism. Her aim was to 'relieve surveys (and the scientific method more generally) from the accusation of positivism' (Marsh, 1979: 293) by showing that that surveys are not 'inherently' positivist and that one does not 'have to buy this unsatisfactory epistemological package to get the free gift of survey methods' (Marsh, 1982: 51). How successful Marsh's attempt to do this was is beyond the scope of the current analysis; however, because of the character of the disputes about positivism and because of the shape they took, the mere assumption that it is possible that surveys and positivism may be inherently related was more likely to leave a legacy than, say, if this possibility was proven ungrounded by sociologists like Marsh.

Therefore, what changed when the disputes about positivism broke out was that quantitatively minded social scientists, such as Goldthorpe, Goldstein and Marsh (effectively the intellectual descendants of Wootton, Carr-Saunders and Glass, whose difficult relationship with sociology was described in previous chapters) *had* to defend, exonerate and justify their work. John Goldthorpe, a prominent sociologist and a strong proponent of quantitative methods in sociology, has described a rather bitter experience with British sociology because of his methodological views. He recalled that since the mid-seventies he has 'felt a complete outsider as far as British sociology was concerned' (Thompson's Interview with Goldthorpe, 2013: 63-4). Another example is Harvey Goldstein, who is not, strictly speaking, a sociologist, but who shared a similar experience, recalling that he

> was labelled as a positivist and that was enough to condemn me – from qualitative researchers…So, positivist: what they meant was someone who just believed in numbers (Author's Interview with Harvey Goldstein, 2017).

Another example is Catherine Marsh – although she has not spoken about being an outsider or being condemned, she was the most vocal and active defender of

the survey method against anti-positivism and, by virtue of the work she was producing, she was herself a small minority.

Inevitably all this led to a situation in which the divide was felt more strongly on an institutional level – only a small minority of sociology departments, such as Nuffield College, Oxford, together with organisations and institutes promoting social research, such as the short-lived SSRC Survey Unit (1970-1976), the Social Research Association, the RSS, the Market Research Society (cf. Marsh, 1980: 4) developed and taught more advanced quantitative methodology as part of their sociology teaching.

While in the early post-war period quantitative sociologists were simply a minority, when the anti-positivist rhetoric was added to the existing theory/method divide, they became a *marginalised* and *ostracised* minority. Those defending the use of survey and statistical methods were being seen as sociologists committing a scholarly 'sin'[43] (Marsh, 1982: 51). This situation emerged regardless of the fact that mainstream sociologists, or sociologists who were part of particular intellectual groups, such as feminism or Marxism or ethnomethodology, made depreciative remarks about survey and statistical methods without having the knowledge necessary for the understanding of even the basic characteristics of such methods.

The widening of the divide between mainstream sociologists and sociologists oriented towards quantitative research and teaching, which resulted from the spread of the disputes about positivism, had pervasive impact on the intellectual culture that characterised British sociology in the late 70s and 80s. The spread of 'anti-positivism' created conditions that reinforced clear anti-quantitative attitudes which were also for the first time examined in more detail.

Discussions from a 1979 conference on the teaching of methodology to sociology students shows clear evidence of this. A number of sociologists with a particular interest in methods described the existence of an anti-quantitative culture on a variety of levels. Burgess and Bulmer argued that a more clearly defined and 'general anti-quantitative culture within British sociology' was emerging in the 1970s and early 1980s (Burgess and Bulmer, 1981: 480). There was weaker

[43] This is Marsh's (1982) description; she was arguing that everyone, even survey researchers are in fact against positivism – hence the association of positivism with sin.

quantitative orientation among sociology graduate students compared to graduate students in other social science subjects (cf. Marsh (1972) in Burgess and Bulmer, 1981: 479); and a lack of articles based on quantitative research in British sociology and a predominance of articles based on non-empirical research (Bechhofer, 1981: 499-500). Other scholars reported in similar vein that there was 'little interest in the potentialities of official statistics for sociological analysis' and 'disinclination to undertake large-scale empirical research' (Bulmer, 1980: 505); and that the teaching of quantitative methods in most departments has been done as 'a concession' (Husbands, 1981: 88).

Coming to the fore in the discussions of the 1970s was a partial explanation that a certain attitude that existed very early on in the post-war period, but which had been more subtly expressed, was becoming slowly and gradually more entrenched in the minds of sociologists and their students. This was a tendency, Bechhofer (1974: 78) observed, 'to regard statistics as something useful to but somehow extraneous to sociology'; as something 'totally separate' and 'optional or of only peripheral importance'.

Empirical, especially quantitative, methods were widely perceived as an exercise in the production and analysis of 'facts about society' by 'social technicians' (cf. Rose, 1981: 515). The idea was 'convincingly argued' that surveys 'are in any case manipulative and convey a calculating bureaucratic stance' (Wakeford, 1981: 509). In the 1970s, sociology had already turned into 'the science of debunking' in which, it was believed, 'no one but the permanent and unremitting critic of society can be an effective sociologist' (Dennis, 1989: 430, 433). Statistical and survey research was unnecessary and unhelpful to such a sociology; and would indicate submission to 'facticity' which was 'one of the tyrannies to be broken' (Dennis, 1989: 434).

Two studies, one in the late 80s and one in the early 90s, confirmed that the production of quantitative research by British sociologists continued to be neglected – Bulmer reported that less than a half (24) of the 52 articles published in the *British Journal of Sociology* in 1986/7 contained systematically collected empirical data but also that, out of these 24, only 8 were by British sociologists (Bulmer, 1989: 394). In 1996, Bechhofer did a more extensive study of the articles in *British Journal of*

Sociology, *Sociology* and the *Sociological Review*, showing that while 51% of the articles were based on the analysis of empirical data, less than one third of them used quantification and the large majority of those who did use quantification, used it at a very elementary level (Bechhofer, 1996: 585). In the end, it all came back yet again to the issue of linking effectively sociological theory and statistical data – the theoretical tradition which expanded during the post-war period did not pay 'much attention to the links with empirical investigation' (Bulmer, 1989: 397) and also:

> far too many sociologists are preoccupied with types of theory which neither derive from problems actually encountered in the conduct of empirical enquiry nor result in propositions of the empirically testable kind (Goldthorpe (1990) quoted in Gartrell and Gartrell 2002: 649)

There were some attempts in the 70s and 80s to redress this situation. As an unprecedented number of young sociologists were graduating and entering the job market, it became an increasing concern that employers demanded graduates that were not only well educated in the 'liberal arts' tradition but also possessed the technical skills to analyse empirical, including statistical, data while very few sociology students, in fact, had such skills. Smith (1975: 312) reported that 'many organisations which offer employment or funds for research workers […] are dissatisfied with the technical competence of sociologists, with a particular problem being 'the low level of expertise in survey method'. But while there was an agreement that the situation of sociology graduates 'would be more competitive if they were more competent in the survey method' (Smith, 1975: 315), there was less agreement on how this situation could be improved.

The SSRC Survey Research Unit had been set up as a way of advancing quantitative sociological research and training; but some, like Bulmer, disagreed that this was a good solution. Initiatives of this kind appeared to be treating the symptoms but not the causes of a much more deeply rooted problem – the gap between theory and empirical methods:

> A proposal to institute survey research training without making sure that an integral part of the course is concerned with how to translate theoretical ideas into researchable problems would seem to run a

> particular danger of emasculating postgraduate work in sociology (Bulmer, 1972: 269).

Although technical competence was necessary, it was a mistake, Bulmer argued to equate methodological training with technical skills training – an important part of the problem was that methods teaching was 'more technical, less intrinsically interesting and something to be tolerated rather than to get excited about' (Bulmer, 1974: 244) which fuelled not only non-constructive disputes about positivism but also a dislike of methods among the students themselves.

The introduction of simply more quantitative training at postgraduate level was also seen as ineffective in tackling another issue which was exacerbated during the disputes about positivism. The majority of sociologists, it was said, 'tend to *favour* different [i.e. non-quantitative] methods' (Bulmer, 1972: 269, *my italics*) – a poignant recognition of the fact that the methodological culture in British sociology was built on *preference* and *belief*; and that unless the power of preferences and beliefs was effectively neutralised, setting up new research units would not do much to overturn the anti-quantitative culture that prevailed in British sociology.

The Legacy of the Disputes about Positivism

How could we explain the association between positivism and statistics that we observe in British sociology in the 1970s? Was it historically inevitable that the disputes about positivism created conditions for an anti-quantitative culture, or could we explain this course of events with reference to some particular characteristics of the post-war development of sociology in Britain?

Oakley's 'make-room' argument about the function of the 'positivist' debates, which I discussed earlier, provides only a partial explanation of the character and impact of the disputes about positivism – it can explain why the disputes took place on a polemical/rhetorical level; why their effect was limited to that sphere and why they took the form of attacks which were largely unsupported by the evidence. However, the 'make-room' argument does not explain why a quantitative, empirical, fact-based, policy-oriented type of sociological/social research *should have become*

the scapegoat in these debates in the first place. In addition, I have shown in previous chapters that quantitative methods, and statistics more generally, did not take up much 'room' in British sociology anyway for them to be shoved aside to make room for anything else. Why then were quantitative methods specifically targeted?

To understand why the disputes about positivism targeted quantitative/empirical/policy-oriented social research, we need to look at the historical conditions that made this choice possible in the first place. A major role in these historical conditions was played by the difficult relationship that British academic sociology had with the empirical, including quantitative, tradition of social enquiry that existed in parallel, but separately, from academic sociology. We have seen that throughout the twentieth century this divide has been a topic for discussion among sociologists as well as an issue they had to deal with during the expansion and in their teaching practice. British sociology has been in its own peculiar way obsessed with quantitative methods, not in the sense of using them unquestionably but in the sense of continually putting forward argument after argument as to why sociology should remain focussed on other things such as theoretical development.

It could be argued, therefore, that it was the separate existence of the empirical tradition and the fact that British sociology had not found an effective way to deal with this separate existence, that created the right conditions for the positivist disputes of the 70s and 80s to target empirical and quantitative social research. The positivist disputes *were* British academic sociology's way of coming to terms and dealing with the parallel existence of the predominantly empirical and quantitative tradition of social enquiry and the lack of success in bringing these two together. While in previous decades this tradition had been either ignored or dismissed on the basis that, in the early stages of the expansion of British sociology, the development of theory was of prime concern, the positivist rhetoric gave sociologists a chance not only to make room for, and associate themselves primarily with, new movements such as feminism and ethnomethodology, but also to dissociate themselves more decisively than before from survey and other quantitative research. Associating empirical quantitative work with positivism provided a better excuse to ignore or reject it.

One way in which this was done was by recasting the empirical tradition of the nineteenth and twentieth centuries as positivist; labelling it as the *origins* of positivism. For instance, in his account on positivism and sociology Halfpenny (1982), following Abrams (1968), makes reference to Galton, Pearson and their eugenics and statistical views as precursors of positivism, while Hobhouse is portrayed as non-positivist because he 'objected strongly to attempts by the eugenicists to reduce the science of society to a part of the science of biology' (Halfpenny, 1982: 39). The nineteenth-century statistical tradition is virtually equated with positivism, the latter being described as a theory of knowledge according to which the natural science of sociology consists of the collection and analysis of quantitative data about society.

Another example comes from the study of positivism in America, France, Germany and Britain by Bryant (1980). In Bryant's analysis of positivism in Britain, the re-casting of the British empirical tradition, and indeed the whole of nineteenth-century British social science, as positivist is clearer still. A great deal of the discussion is also based on Abrams' account of the origins of British sociology, with Bryant describing the empirical tradition of social enquiry as positivist due to the 'predominance of individualism', 'prominence of abstracted empiricism in statistics and surveys'; belief that knowledge of the social conditions is 'of practical utility to the causes of administrative reform' (Bryant, 1980: 108). These were almost verbatim the descriptions coined by Abrams in 1968; the difference being that in 1980 they were *no longer merely critiques,* they were *the basis for an anti-positivist critique.* This opinion persisted throughout the 1980s, with Albrow, who had been a BSA president in 1986, arguing in 1989, that there has been 'a persistent empiricism and positivism in the British approach' since the eighteenth century' (Albrow, 1989: 214).

It is through historical readings such as these that the existence of an anti-positivist movement targeting quantitative methods was justified and the gap between the two traditions – academic sociology and empirical social science – was legitimised. In associating what they understood as positivism with the empirical and quantitative tradition of social enquiry sociologists found a way to relieve a tension that had existed for a long time.

But why was in the late 70s and 80s a stronger rationale for discarding empirical and quantitative research necessary? Could mainstream sociologists not object to quantitative methods without invoking their apparent association with positivism as justification?

A stronger rationale was partly needed to resist numerous attempts by the SSRC to encourage the merging of the two traditions by funding empirical and quantitative research within sociology; something with which many sociologists strongly disagreed. During the 1970s, many sociologists developed a negative attitude towards the SSRC. They argued that the SSRC 'always talked as though social research deals with politically unproblematic policy questions' (Rex, 1978a: 415) and promoted 'old-fashioned forms of research design' (Rex, 1978b: 296); therefore, the SSRC was 'resented for the influence it does exercise by many sociology teachers' (Rex, 1978a: 415). More evidence of the dissatisfaction of sociologists with the SSRC comes from Mack (1979: 15-17) who examined the problematic aspects in its structure and initiatives, outlining the disparity of opinions between mainstream sociologists and the SSRC itself. And there are a number of contemporary and more recent accounts that describe the SSRC as having a 'bureaucratic preference for positivism' (Ditton and Williams, 1981: 11); as biased 'towards a positivist conception of social science research', a clear example of which was, supposedly, Lord Heyworth's emphasis in his 1965 report on the necessity of more and more adequate statistics training (King, 1997: 2, 21). The SSRC was seen as an institution that, due to its financial incentives, offered complete obedience to an overarching science and technology outlook, and therefore privileged 'positivist social science' (Donovan, 2005). Donovan even went as far as describing the early Clapham (1946) and Heyworth (1965) reports as having produced 'a bias towards positivistic scientific methodologies which were seen as untainted by ideology' (Donovan, 2005: 605) regardless of the fact that these reports were produced in a completely different social and political climate in which positivism was not an issue. Donovan's arguments appear to be largely based on a misreading of the Clapham's and Heyworth's reports, attributing statements to them which are not present in the original text; and paying little heed to the wider historical – social and political – context in which the Clapham and Heyworth reports were produced.

But while mainstream sociologists were criticising the SSRC for *making attempts* to encourage more empirical, policy-oriented and quantitative research, others such as Marsh criticised it for not doing enough in this direction, especially in promoting the development of *theoretically relevant* surveys:

> Despite the establishment of the SSRC in 1965 and despite the enormous growth in the volume and sophistication of survey research in Britain and abroad during that period, the improvement that this should have made possible in empirical studies of sociological nature has not occurred. If anything, the number of theoretically relevant surveys has declined and the interest of sociologists in surveys has suffered an eclipse (Marsh, 1980: 2).

Whether the SSRC was indeed biased towards one type of research, or not, is beyond the scope of this thesis. But the examples above make clear that a variety of groups of sociologists – those who were anti-positivist, survey researchers and those who have studied the SSRC – expressed a lack of satisfaction with the way the SSRC functioned. As Bulmer (1980: 508) argued, 'the world is not made-up just of knowledgeable sceptics and naïve hard-line positivists'. However, the disputes about positivism created the impression of a black and white world – this is obvious from the way the SSRC, and the empirical tradition of social enquiry, were enmeshed in these disputes.

In sum, the disputes on positivism did not have an effect of decreasing the amount of teaching of quantitative methods; but they were powerful enough to instil a belief, a certain attitude among sociologists, that using these methods was somehow 'un-sociological' or *insufficiently* sociological: sociology students, *developing under the influence of teachers who held such beliefs*, emerged unconvinced that 'empirical work is still necessary, possible and enjoyable' (Rose, 1981: 517). The legacy of these disputes lies in creating suitable conditions for the emergence and sustaining of an anti-quantitative culture; and in providing an influential, albeit questionable, epistemological justification of why British sociology *could* afford to disregard quantitative methods teaching and research and continue to exist in parallel to the more quantitatively oriented social science that was practised elsewhere.

But these debates also had the effect of turning the lack of effective incorporation of quantitative methods in British sociology into a topic of serious discussion. It was now for the first time vocally argued that dismissing quantification would inevitably harm any empirical research, since 'most forms of empirical research involves statistical principles in a fundamental way' (Bechhofer, 1974: 78). Such a position 'can only be maintained if sociology is to be regarded essentially as a form of social philosophy' (Bechhofer, 1974: 78); and if sociology teachers are content with producing 'half-sociologists' (Author's Interview with Robert Moore, 2017).

Along with this came a clearer understanding of statistics that went beyond the technical skills involved in applying statistical methods in sociological enquiry. Learning how to produce and analyse statistics is not simply a matter of acquiring technical expertise, it is a necessary skill for understanding change in society and for being an intelligent citizen:

> If you don't acquire some capacity to handle numbers with a degree of confidence, you are going to be a victim, so for heaven's sake, lets get on top of this and learn how to appreciate the numbers [...] So I would say to students, this is in a sense nothing to do with sociology, it's to do with citizenship and being a member of society (Author's Interview with Robert Moore, 2017).

And so, the discussions in the 1970s and 1980s made it clearer that the problem with the relationship between British sociology and statistics was not merely the problem of failing to produce technically skilled social statisticians, but, more significantly, the problem of failing to produce sociologists who appreciate the value of statistically informed judgment, within or outside of their subject and beyond the technicalities; not merely as sociologists, but as democratically engaged citizens.

Conclusion

'Sociology', wrote Malcolm Bradbury in 2012, 'had a glorious heyday in the sixties and then began to fragment and die – not as a discipline among others, but as the great discipline, the key to all knowledge' (Bradbury, 2012 [1975]: 250). Bradbury wrote these words in an apologetic post-script to his satirical novel, *The History Man*, whose infamous character, the sociologist Howard Kirk, had his small share in bringing about British sociology's 'fall from grace' in the late 70s. Despite the fact that the popularity and public image of sociology in Britain have since largely recovered, Howard Kirk, the intellectual rogue riding on the crest of history, has remained one of the best-known stereotypical faces of British sociology.

But Malcolm Bradbury was not the first novelist to make an assault on social science and Howard Kirk was not the first grotesque personification of a social science subject. Some hundred and twenty years previously, Charles Dickens also relied on the psychology of stereotypes in his novel *Hard Times* (1854) to satirise the burgeoning statistical movement through the character of Thomas Gradgrind. Mr Gradgrind was not a lecturer like Kirk, but a relentless school teacher, guided by the 'soulless' philosophy of facts, constantly reprimanding his pupils for failing to see that 'in this life we want nothing but facts'. Gradgrind's fanatical lessons in 'fact-ology' easily overshadow the London Statistical Society's zealous tirades about *aliis exterendum* discussed at the beginning of this thesis. But despite his extreme features, Gradgrind's character continued to dominate the perception of statistics for the majority of British sociologists throughout the twentieth century.

Grandgrind and Kirk have little in common: Gradgrind being the hard-headed and emotionless fact-cruncher; and Kirk being the radical and complacent socialite, indulging in both passion and intrigue. As stereotypical, representative figures however, both served one and the same purpose – to distort, exaggerate and mock the image of the subjects they personify; what Gradgrind did for the image of statistics in the 1850s, Kirk did for the image of sociology in the 1970s. But it is not only literature that has viewed statistics as Gradgrind and sociology as Kirk. As this thesis has shown, sociologists and statisticians themselves have often seen each other in terms of these stereotypes.

This thesis has attempted to expose the harm caused by applying the psychology of stereotypes to understand the historical relationship between statistics and sociology in Britain. By redressing some of the misunderstandings and dispelling some of the myths that surround the history of both subjects, it has attempted to show that statistics and sociology have had many faces, not just Grandgrind's and Kirk's; and that some of the 'faces' of British sociology can only be seen when we also look at the 'faces' of statistics.

Looking at the historical development of both subjects simultaneously, we discovered an issue which is, at the very least, puzzling: how was it possible that in the country that spearheaded the development of modern statistics, academic sociology has not only consistently resisted the incorporation of statistical thinking and methodology but has actively defended and promoted its own a-statistical character?

British sociologists have been effective in their use of the nineteenth-century Gradgrind image of statistics to justify the divide between academic sociology and a statistically based social science that developed elsewhere. Abrams' 1968 essay on the origins of British sociology, discussed at the beginning of this thesis, is a good example. Abrams argued that the statistical and social reformist movements that developed in Britain in the nineteenth century were a failed start for social science: statistics was a Gradgrindean exercise in mindless number crunching; while social reformist research was limited to piecemeal empirical investigations. In his view, academic sociology had little to gain by investing time and effort in the incorporation of such types of social enquiry; what's more, the fact that these types of enquiry developed institutionally, stifled any possibilities for the emergence of 'sociology' which Abrams saw as primarily oriented towards providing a theoretical insight into social development and processes.

Parts of the statistical and social reform movements were, indeed, as Abrams described them. However, the first part of this thesis showed that Abrams' description is limited by his failure to take account of the intellectual context in which statistics developed historically; and distorting because of his arbitrary assumption that a primarily theoretically oriented social science was the only right and proper path for sociology in Britain. However, the aims and structure of the

statisticians' work in the early nineteenth century, as well as their own understanding of their contribution to knowledge, were closely related to the aims and understanding of science at that time. Placed in the context of the contemporary understanding of science, the events and discussions that took place in the two most prominent statistical organisations, Section F of the BAAS and the SSL/RSS, are an example of the very first attempts to establish precise, reliable and widely applicable knowledge about a constantly changing, complex and unpredictable social world. But why is it important that statistics was part of a wider scientific development; and why does it matter for our historical understanding of the relationship between statistics and sociology?

There is extensive scholarship on the origins of modern science and the Scientific revolution (Butterfield, 1949; Jardine, 2000; Jacob, 2010; Knight, 2014; Wootton, 2015); few accounts, however, have come up with a convincing explanation of the *conceptual* changes, the watershed moments in humankind's *worldview*, that have given rise to modern science. A recent book that stands out in this respect is David Wootton's *The Invention of Science* (2015). Wootton argues that the discovery of the 'New World' by Columbus in the late fifteenth century was *the* motive force that convinced many European scholars of the real possibility of going beyond the great discoveries and monumental intellectual achievements of the ancients, whose thinking had dominated centuries, and embrace a radically novel view that creating new knowledge and transgressing intellectual boundaries is not only conceivable, but necessary. Columbus' discovery is, of course, not directly responsible for the Scientific revolution that followed in the sixteenth and seventeenth centuries but it showed that the ancients did not know everything, that they could be surpassed and it unleashed the power of scientific imagination. Is there an equivalent moment in the history of social science? And if so, who is the Columbus that provided the inspiration?

The Renaissance and Enlightenment periods contain many prominent examples of philosophical scholarship relating to the development of society and various social matters. With few exceptions, however, these are based on deductive reasoning and abstract principles derived from certain assumptions about human nature. Although the social philosophy of scholars like Thomas Hobbes, John Locke, Jean Jacques

Rousseau and Adam Smith have provided valuable insights into some of the major principles underlying the workings of society, social *science* – the systematic investigation of social matters and their explanation on the basis of empirical evidence – only began in earnest in the nineteenth century. This was also the time when the development of natural science took a particularly important turn. What is special about this period was that discoveries and inventions in the fields of physics, chemistry, geology and biology triggered fundamental changes in the organisation of social life that were unprecedented, creating conditions for the radical industrialisation and urbanisation of society, especially in Britain. This, in turn, stimulated further scientific developments to help manage social life, for instance, in the fields of medicine, sanitation and technology. Thus, advances in science changed society and changes in society stimulated further scientific advance: the interaction between science and society during the nineteenth century was more intense and on a larger scale than ever before.

It was in this climate that a genuinely novel problem emerged – how to even begin to study a society with an enormous amount of variability of social interactions and conditions which was rapidly becoming ever more complex? To solve this problem, one needed a new approach to studying society – a social *science* that is capable of harnessing social heterogeneity. A social philosophy based on social types or principles of human nature would have been ineffective for this purpose.

There was one development which, more than anything else, helped in the actual realisation of the idea that a social *science* is possible – the rise of modern social statistics, which manifested itself most distinctly in Adolphe Quetelet's invention of the Average man. The Average man was a 'personification' of the idea that data about human beings can be summarised in a normal distribution and used for empirical social analysis – that one can gain information by discarding information (Stigler, 2016). It was the systematic collection of numerical data and their analysis through the technique of averaging, to begin with, that brought about a conceptual change in Western societies not only in the way they understood society but also in the way they understood *how* to study society. Just like natural science, statistical social science offered unique access to knowledge about the world that could not be achieved *in any other way*. What Columbus, in his discovery of the

New World, did for natural science; Quetelet, in his invention of the Average man, did for social science.

This, of course, was just the beginning – neither Columbus, not Quetelet could have imagined the scale of the transformation that their discoveries were to bring. Just as Columbus believed, wrongly, that he had reached India and not a completely new continent; Quetelet believed, wrongly, that his Average man was a fictitious being encompassing all that is good about the human species. But to dismiss the importance of *their* discoveries due to their limited understanding of what it was that they had discovered and of the potential consequences of their discovery would be to impose limits on our *own* understanding.

Statistics is not merely a set of techniques among many others; it is a worldview that came with the development of modern science and which forms the basis of an approach to social enquiry that most adequately captures the complexity, the variability and the constant change that characterises the basis of modern society. Statistical enquiries cannot serve as a replacement for other theoretical enquiries or enquiries based on the qualitative analysis of non-numerical data about society; statistical enquiries can, however, provide these with a firm base.

It is precisely because of the historical role of statistics in the history of science, including social science, that the historical relationship between statistics and sociology in Britain *matters*. And it is because of the historical role of statistics in the history of science that it is misleading to describe nineteenth-century statistics as a 'frustrating' factor in the development of British sociology, as Abrams does. And, unlike Abrams' account which is based, albeit implicitly, on the arbitrary assumption that the right path for sociology is the theoretical investigation of society, the long-term profound consequences that the development of statistics has had for understanding society and social change provides us with a legitimate historical reason to *question* the course of events that led to statistical social science and academic sociology in Britain taking different paths.

The first part of this thesis showed that the nineteenth century gave rise not only to the statistical movement, manifested in the work of the SSL/RSS and, later on, Galton and Pearson and their followers; but also to the sociology of Comte and Durkheim which was not blindly followed in Britain but which, nonetheless, served

an important role in showing that *it is possible to create a sociology of a non-quantitative type in which the main and overriding element is theory of society.* It was not that both traditions were somehow inherently incompatible; what was crucial was that both traditions perceived each other as mutually *irrelevant*.

When the Sociological Society was established in the early twentieth century, it had a choice between two distinct paths for its development – was it going to choose to follow in the steps of only one of these traditions or create a bridge between them? The second part of this thesis traced the choices made by the Society with regard to the establishment of academic sociology in this country and its relationship with the statistical tradition of social enquiry. It showed that the Society achieved limited success in its attempts to define the goals and methods of sociology; to establish it on a firm basis in academia and raise its status among other groups of social scientists. There were also a variety of factors responsible for the failure of the Society to establish a statistical basis for sociology in Britain, including the Comtean understanding of sociology and methodology that they embraced and the preferences of its leading members for non-quantitative types of sociological enquiry. The absence of anyone in sociology with a more advanced understanding in statistics and a devotion to its incorporation into the academic sociology as well as the close relationship that statistics had already developed with economics, also played their part. Once sociology was associated with a particular choice of words, of thought, of institutions established by the Society, *regardless of whether these choices bore fruit or not*, it was very difficult for sociology to re-organise itself into something different.

The examination in the second part of the thesis of the relatively stagnant development of sociology in the interwar period showed that there were few opportunities, if any, for the incorporation of statistics into the kind of academic sociology practised by sociologists such as Hobhouse and Ginsberg, after the Sociological Society secured sociology's place at the LSE. This was not because they promoted a sociology which was *explicitly* hostile to statistics but because they perceived statistics as irrelevant, insisting that the true value of sociology lies in the realm of broad theoretical insight, far beyond anything that could be achieved

through mere work on 'drink, drainage and divorce' – the derogatory term used to describe statistical enquiries.

While sociology struggled to escape its marginal position in academia in the first half of the twentieth century, the application of the new English statistics spread in both the social and natural sciences while the empirical tradition of social enquiry that originated with the work of William Farr, Edwin Chadwick, Charles Booth, Seebohm Rowntree and others was being established, separately, in social science departments in universities, the most prominent example being the SSaA department at the LSE. How was it possible *at all* then that an explicitly non-statistical and non-empirical academic sociology could have emerged and sustained itself in an intellectual climate in which the statistical movement and social empirical enquiry were both gaining ground? How was it possible for an academic sociology in this country to continue rejecting statistics which had given rise to social science in the first place and continue to exist, let alone thrive? If most of the empirical social science in the nineteenth and twentieth centuries occupied the space for social science created by the discovery of the potential of statistical data to make society scientifically knowable, then what space did 'sociology' occupy and how was this space created and sustained?

The conceptual breakthroughs brought about by statistics made social *science* possible; they also made largely redundant the contributions of social enquiries that did not make *any* use of quantification; but they did not eliminate the possibility for the existence of a type of enquiry claiming to offer insight about the social world that went *beyond* anything that could be discovered through statistical analysis. Thus the nineteenth-century social science based on statistical analysis, by failing to fill the space itself, left room for the social philosophy of the type that existed before the discovery of modern statistics. And it is this space which Comte's philosophy, Durkheim's sociology and eventually, the whole of British academic sociology, came to occupy. British sociology cultivated room for its existence *on the basis of its opposition* to statistical social science and its affiliation with the type of knowledge which was, essentially, social philosophy. Disassociating itself from the methods of statistical social science became one of its hallmarks and distinguishing features used to justify its separate existence.

This is most clearly pronounced in the intellectual and institutional development of sociology in Britain after 1945 which I examined in detail in the third part of this study. I showed that British sociology in the post-war period was dominated by a continued interest in theoretical enquiry, despite the fact that post-war sociologists distanced themselves from the theoretical work of their British predecessors and embraced many of the post-modern theories first developed in America. More qualitative empirical work *was* being done during this period, but it was characterised by an even stronger and clearer rejection than before of statistical social research which was considered un-sociological or insufficiently sociological, in spite of the fact that there was still little clarity as to what 'sociology' itself actually was.

It was the expansion of the higher education system in a particular context of social upheaval, desire for change and break with the norms and traditions of the past, political reorganisation and suspicion of science that created favourable conditions for the expansion of social studies more generally. Although sociology expanded more quickly and at a larger scale than any other social science subject, what's crucial about the expansion of *sociology* is not so much its scale but the fact that what made it possible in the first place were *external* factors having little to do with sociology's own intellectual or institutional progress. What in the early twentieth century looked like sociology's greatest disadvantage – its lack of a clear sense of direction – in the post-war period turned out to be its greatest advantage as sociology could adapt to the institutional, social and cultural trends and make itself popular without having to conform to any pre-existing constraints (such as a well-rooted and dominant tradition or institutions). It was during this period that sociology established a strong reputation as a socially relevant, cutting-edge subject that appealed to students who aspired to challenge the *status quo* and change society for the better. But, crucially, sociologists consistently emphasised that this could, and even should, be done on the basis of sociological theory, not empirical, let alone numerical, data on society. Attacks on social surveys based on bad quality research conducted in America helped to fuel the perception that statistics would be useless for achieving the goals that sociology set for itself; Howard Kirk was 'the history man' because he was *above* statistics.

The expansion had an unquestionably positive effect on the institutional development of British sociology as an academic subject. The teaching of sociology became part and parcel of the programme of almost every university in the country and diversified both methodologically and substantively. This thesis examined teaching trends in much greater detail than trends in research due to the vital influence teaching has on shaping generations of students as well as prospective teachers and researchers and thereby the entire future of an academic subject.

The analysis of the teaching trends at the LSE and also universities across the country showed that the teaching of statistics remained consistently at a basic level. Furthermore, the evidence discussed in Part Three showed clearly that sociologists rarely showed appreciation for the wider potential of statistics, as a result of which the teaching of quantitative methods to generations of sociology students was compromised. The teaching of quantitative methods has never been totally absent from academic sociology; on the contrary, up to the 1980s these methods were regularly part of the methodology curriculum. However, among British sociologists, statistics has consistently been seen, at best, as a specialist, esoteric skill and, at worst, as a dangerous and fruitless path that would bring down sociology, condemning it to a life 'in the sands', to use T. H. Marshall's phrase. This has been the on-going attitude of *both* those who argued that sociology is a science and attempted to work towards making it scientific (like Comte, and his followers in the Sociological Society) and those who argued against closer affiliation with science as in academic sociology mostly from the 1960s onwards. In addition, where the option of teaching statistics was embraced, this usually took place in an intellectual climate in which sociology more generally was conceived as subject aimed at providing a general education and in which the development of sociological theory was seen as the priority. Given that no special efforts were made in squaring sociologists' intellectual ideals and epistemological concerns with the aim of teaching students how to become practically skilled in quantitative methods, the teaching of these methods, although consistently present in sociology, itself became an opportunity to broaden students' general knowledge, instead of enhance their practical empirical skills.

The analysis of the methodological discussions that took place in the post-war period showed that often choices about what was taught in British sociology – especially methods teaching – were influenced by concerns of what would sustain the subject's popularity; and often choices to make quantitative methods teaching optional or leave it to a post-graduate stage have been dependent on concerns about the image and popularity of the subject and student demand. But British sociology's institutional stability could not totally compensate for its intellectual frailty which resulted from the fruitless attempts of sociologists in the post-war period to agree on the methods and aims of sociology. There is scope to argue that a good part of that frailty was a result of the fact that sociology in Britain has remained separate from much of the empirical, including statistical, tradition of social enquiry which itself rests on a firm intellectual basis laid by the founders of social science, such as Quetelet.

This thesis has shown that the issue at the core of the relationship between statistics and sociology is not that there have been *some* British sociologists that have ignored statistics; or that there have been *some* areas of sociological enquiry where statistics could have been used with success but were not. The issue at the core of the historical relationship between sociology and statistics in Britain is that quantitative methods and the probabilistic understanding of society, and the statistical worldview more generally, have *consistently* and *systematically* been ignored or rejected by the *majority* of British sociologists[44]. Although this course of development was *not inevitable,* this thesis showed that, overall, since academic sociology was established in Britain at the beginning of the twentieth century it evolved as if the Average man had never been invented.

One possible way to explain this divide in British social science would be to argue that since social statistics developed early in the nineteenth century and quickly established its own institutions and, throughout the twentieth century, was central to the work in other social departments, government institutes etc., academic sociology decided not to trespass cultivated land and instead establish itself in new territory pursuing a different type of social enquiry. The evidence examined in this

[44] Except for a small minority at Nuffield College and departments such as Essex and Southampton, which developed strong quantitative research programmes.

thesis did not reveal any intense direct conflict between the statistical social science that developed in Britain out of the work of the SSL/RSS and, later on, Galton and Pearson's 'new English statistics' and continued to be practised in social science departments and institutes; and the type of non-quantitative and largely non-empirical sociology that developed academically in Britain. There was simply little interaction between these two 'traditions' on both institutional and intellectual levels. There were occasional outbursts of hostility against the teaching and use of statistics in social enquiries on the part of academic sociologists, especially in the post-war period, but these did not represent attacks directed at any particular body of statistical work existing outwith academic sociology. Given their overwhelming lack of statistical skills, sociologists have rarely been in the position to mount such attacks effectively. So, indeed, at a first glance it may look as if the divide between academic sociology and the rest of social science which had a statistical basis was an unproblematic development.

Had this been the case, however, then it becomes more difficult to explain why sociologists made consistent efforts to excuse themselves from using statistics and justify their efforts in this respect. Overall, the evidence showed that broad and mostly impressionistic criticisms of statistics took place consistently *within* British academic sociology, with a majority of sociologists arguing against the need for quantitative methods teaching and the use of such methods in research; and a minority arguing for the need for sociology to embrace statistics. If the divide was a solution to sociology's problem of finding its own independent place in academia, then it would also become difficult to explain why the anti-positivism debates took root and spread so widely among sociologists since they were a clear manifestation of yet another attempt by sociologists to resolve among themselves the issue of their lack of engagement with statistics by arguing that any use of statistical evidence for sociological analysis would make this analysis 'positivist'.

If, therefore, there has been a conflict in the history of British sociology with regard to statistics, this was conflict *within* British sociology itself aimed at resolving the perennial questions of what exactly sociology is and how it should position itself in the intellectual conditions created by the invention of statistics, the very thing it was consistently rejecting. Thus, and somewhat paradoxically, while sociology in

Britain was struggling to define its object and methods of study, it exhibited such an obsession with quantification in the form of its numerous attempts to justify its lack of engagement with statistical methods and outlook, that this obsession, it could be argued, defines its character better than anything else. While British sociology has not yet made up its mind what it is; a long time ago, it seems, as this study has shown, it decided what it is *not*.

The evidence presented in this thesis shows that the issue dividing sociology and statistics in Britain in the nineteenth and twentieth centuries has been of an epistemological and philosophical nature (rather than a practical one). This means that the lack of success in incorporating statistics into sociology was not the result of a failure to organise, in a practical way, the teaching of statistics in the sociology curriculum or secure funds for statistically based research. It was largely the result of a conviction, prevalent among British sociologists, that sociology and statistics are not epistemologically compatible. Statistics aspires towards objective knowledge, it relies on evidence and is based on the assumption that there is such a thing as facts and that *both* the social and the natural world can be described using mathematics. Sociologists, on the other hand, have argued that social reality is far too complex to reduce to numbers and mathematical relations; that there is no such thing as objective knowledge; that knowledge about society is socially constructed and relativist; and that the natural world and the social world are separate and distinct entities which require separate and distinct research methods. These may be sound epistemological concerns; however, it is questionable to what extent British sociologists were in the position to defend them, given that the widespread criticism towards statistics among British sociologists was based on general ignorance of statistical methods and impressions based on cherry-picked examples of bad statistical research, usually from the USA.

The long-term, historical relationship between statistics and sociology in this country has been weak, troubled; at times, painfully antagonistic. There is good reason to believe that it has stifled the development of a healthy and vibrant social science in this country. Despite the assertions and belief of some that the relationship is of little concern, indeed irrelevant, the core aim of this study has been to show that it is an issue central to British sociology and of significance to all sociologists, not

merely to sociologists interested in statistics. On a micro level, this study has led us to question the choices made by particular people; to probe the factors responsible for the establishment and development of sociological institutions which were not receptive to the idea of developing close ties between sociology and statistics in this country; it has led us to revaluate particular teaching practices, to expose underlying attitudes and prejudices and to debunk some common myths. But on a macro level, this has been a study about the relationship of sociology with science and how sociology in this country has reacted to the conditions created by a modern science built on statistical foundations. Sociologists have been consistent in their insistence that the social world cannot be understood with numbers alone and only rarely have they acknowledged that the social world cannot be understood *without* numbers.

The main purpose of this study has not been to condemn such ways of thinking but certainly to open eyes and open minds as to how, historically, such ways of thinking came to be seen as right. By doing so, it revealed that there were other possibilities, other ways of thinking, other possible paths that, had circumstances been different, might have been followed. This process of 'opening up', however, is not just about the past, about what has been and gone, it is about the future, about re-evaluating the road we are on and the direction in which we are heading. Equipped with the knowledge and understanding that this study has tried to provide, we can, hopefully, with eyes wide open, see more clearly the alternatives that lie ahead.

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Appendix I

Topic	1834-1843	1844-1863	1864-1883	1884-1908
Moral and Social	37 (3)*	32 (4)	40	34
Vital	25 (2)	53 (2)	24 (1)	43
Industrial	10 (3)	8	8	
Political	10	15 (5)	16	14
Commercial	2	17	21	12
Financial		27 (1)	32 (2)	21
Agricultural	-	-	-	15
Production	-	-	-	12
Labour	-	-	-	15
Price	-	-	-	5
Transport	-	-	-	15
Methodology	6	2	6	12
Miscellaneous	13 (1)	29 (1)	45	9
TOTAL	103 (9)	183 (13)	192 (3)	207
Other contributions towards methodology	2 (Porter and Guy, 1839-40)			

Table 1. Articles by topic published in the *Journal of the Statistical Society of London (Journal of the Royal Statistical Society)*, 1834-1908. *Source:* Bonar and Macrosty (1934).
* The numbers in brackets represent papers not printed.

	1938-9	1953-4	1958-9	1961-2	1963-4	1965-6	1966-7	1967-8	1968-9	1969-70	1970-1	1971-2
Oxbridge	21.9	18.4	17.5	15.7	14.3	11.6	11.0	10.2	9.9	9.7	9.6	9.3
London	26.4	22.5	21.0	20.0	19.0	15.6	15.8	15.3	14.9	14.4	14.5	14.3
Other	26.1	36.2	38.6	40.7	42.7	49.5	50.6	52.1	52.8	53.5	53.4	53.9
Wales	5.5	5.5	5.8	6.4	6.7	6.6	6.5	6.4	6.4	6.4	6.5	6.3
Scotland	20.1	17.4	17.1	17.2	17.3	16.7	16.1	16.0	16.0	16.0	16.0	16.1
TOTAL %	100	100	100	100	100	100	100	100	100	100	100	100
TOTAL	50,000	80,602	100,204	113,143	126,445	169,486	184,799	200,121	211,485	219,506	228,131	234,985

Table 2. Regional distribution of full-time undergraduate and postgraduate students. *Sources:* University Grants Committee. *University Development, 1962-1967. Cmnd 3820*. London: HMSO; University Grants Committee. 1953-1964. *Returns from Universities and University Colleges*. London: HMSO ; University Grants Committee. 1966-71. *Statistics of Education, vol. 6 Universities*. London: HMSO.

Notes: Statistics on regional distribution after 1971 are unavailable in *Statistics of Education vol. 6 Universities*.

	1919-20	1938-9	1953-4	% change
Pure Sciences	17.7	15.3	21.1	3.4
Technology, Applied Science	14.9	10.6	12.4	-2.5
Medicine and Dentistry	34.1	27.3	19.6	-14.5
Agriculture	1.6	2.1	2.6	1
Vet Science	-	-	1.3	-
TOTAL Arts and SS %	31.7	44.7	43	11.3
TOTAL Science %	68.3	55.3	57	-11.3
TOTAL All Students %	100	100	100	
TOTAL All Students	-	50,002	80, 602	

Table 3. Distribution of all students by subject. *Source*: University Grants Committee. 1957-1961. *University Development: Interim Report on the Years 1957-61*. Cmnd.1691. London: HMSO, p. 83 and p.179.

	1960-1	1961-2	1962-3	1963-4	1964-5	1965-6	1966-7	1967-8	1968-9	1969-70
Social Science	11.3	11.2	11.7	12.5	13.0	15.8	21.3	20.9	20.8	20.6
Arts	30.8	30.9	30.7	30.1	31.3	28	20.9	21.8	22.5	23.7
Pure Sciences	23.8	24.6	26	26.4	25.6	22.5	25.7	25.9	25.5	24.5
Technology, Applied Science	15.1	15.2	14.2	14.4	14.3	19.5	16.6	16.5	16.5	16.5
Medicine and dentistry	15.9	15.1	14.5	13.9	13.3	10.7	11.8	11.4	11.1	11.0
Agriculture	1.8	1.8	1.7	1.6	1.5	1.3	2.1	1.9	2	1.9
Vet Science	1.3	1.2	1.2	1.1	1	0.9				
TOTAL Arts and SS	42.1	42.1	42.4	42.6	44.3	43.8	42.2	42.7	43.3	44.3
TOTAL Science	57.9	57.9	57.6	57.4	55.7	54.9	56.2	55.7	55.1	53.9
Architecture and Other Vocational Subjects	-	-	-	-	-	1.3	1.6	1.6	1.6	1.8
TOTAL UG students %	100	100	100	100	100	100	100	100	100	100
TOTAL UG students	89,863	93,781	98,211	103,890	113,144	140,179	152,230	164,653	173,510	180,179

Table 4. Distribution of full-time undergraduate students by subject, 1960-1969. *Sources*: University Grants Committee. 1960/1-1965/6. *Returns from Universities and University Colleges*. London: HMSO; and University Grants Committee. 1966-78. *Statistics of Education, vol. 6 Universities*. London: HMSO.

Notes:
Separate statistics for the distribution of *undergraduate* students by subjects and separate statistics for arts and social science students are not available prior to 1960-1.
For the period 1966-7 to 1978-9, Social Science includes: Business management studies; Economics; Geography; Accountancy; Government and public administration; Law; Psychology; Sociology; Social anthropology; Combinations between the social science subjects; and combinations of social science subjects with ''Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
For the period 1966-7 to 1978-9 'Arts' include: 'Education', 'Language, Literature and Area studies' and 'Arts other than Languages'.
Totals include: all undergraduate students studying for "First degree" *and* "First diploma" *and* "Courses not leading to a qualification". The majority of students who entered higher education in this period studied for a first degree.

	1970-1	1971-2	1972-3	1973-4	1974-5	1975-6	1976-7	1977-8	1978-9
Social Science	20.8	21.3	21.6	22.1	22.7	23.6	24.1	23.9	23.9
Arts	23.7	22.9	22.7	22.9	23.2	23.3	23.3	23.5	23.5
Pure Sciences	24.6	25.1	25.4	24.9	24.3	23.4	22.7	22.7	22.8
Technology, Applied Science	16.3	16.0	15.6	14.8	14.3	14.1	14.5	14.8	14.9
Medicine and Dentistry	11.1	11.1	11.4	11.9	12.0	11.9	11.7	11.4	11.3
Agriculture / **Vet Science**	2.0	1.9	1.8	1.8	1.8	1.9	1.9	2.0	2.0
TOTAL Arts and SS	44.5	44.2	44.3	45.0	45.9	47.0	47.5	47.4	47.4
TOTAL Science	54.0	54.1	54.2	53.4	52.4	51.4	50.9	50.9	51.0
Architecture and Other Vocational Subjects	1.5	1.7	1.5	1.6	1.7	1.7	1.6	1.7	1.6
TOTAL UG Students %	100	100	100.0	100.0	100.0	100.0	100.0	100.0	100.0
TOTAL UG Students	185,872	190,493	189,938	193,906	199,461	208,065	217,835	227,988	235,057

Table 4 (continued). Distribution of full-time undergraduate students by subject, 1970-78. *Sources*: University Grants Committee. 1960/1-1965/6. *Returns from Universities and University Colleges*. London: HMSO; and University Grants Committee. 1966-78. *Statistics of Education, vol. 6 Universities*. London: HMSO.

Notes:
Separate statistics for the distribution of *undergraduate* students by subjects and separate statistics for arts and social science students are unavailable prior to 1960-1.
For the period 1966-7 to 1978-9, Social Science includes: Business management studies; Economics; Geography; Accountancy; Government and public administration; Law; Psychology; Sociology; Social anthropology; Combinations between the social science subjects; and combinations of social science subjects with ''Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
For the period 1966-7 to 1978-9 'Arts' include: 'Education', 'Language, Literature and Area studies' and 'Arts other than Languages'.
Totals include: all undergraduate students studying for "First degree" *and* "First diploma" *and* "Courses not leading to a qualification". The majority of students who entered higher education in this period studied for a first degree.

	% sociology of social science UG students	% sociology of all UG students	% economics of social science UG students	% economics of all UG students	% social science of all UG students	% social science of 'Arts and social science' UG students	% arts of all UG students	% arts of 'Arts and Social Science' UG students	% arts and social science of all UG students	% pure science of all UG students	% all science of all UG students
1960	-	-	-	-	11.3	26.8	30.8	73.2	42.1	23.8	57.9
1961	-	-	-	-	11.2	26.6	30.9	73.4	42.1	24.6	57.9
1962	-	-	-	-	11.7	27.6	30.7	72.4	42.4	26.0	57.6
1963	-	-	-	-	12.5	29.3	30.1	70.7	42.6	26.4	57.4
1964	-	-	-	-	13.0	29.3	31.3	70.7	44.3	25.6	55.7
1965	-	-	-	-	15.8	36.1	28.0	63.9	43.8	22.5	54.9
1966	8.5	1.8	10.2	2.2	21.6	51.2	20.6	48.8	42.2	25.7	56.2
1967	8.7	1.8	11.7	2.5	21.1	49.5	21.6	50.5	42.7	26.0	55.7
1968	9.5	2.0	11.8	2.5	21.0	48.6	22.3	51.4	43.3	25.6	55.1
1969	9.8	2.0	12.4	2.5	20.5	46.2	23.8	53.8	44.3	24.3	53.9

Table 5. Distribution of students by subject, 1960-9. *Sources*: University Grants Committee. 1960/1-1965/6. *Returns from Universities and University Colleges*. London: HMSO; and University Grants Committee. 1966-69. *Statistics of Education, vol. 6 Universities*. London: HMSO.

Notes:
The figures for % change of sociology and economics students of all UG students is for the period 1966-1978.
Social Science includes: Business management studies; Economics; Geography; Accountancy; Government and public administration; Law; Psychology; Sociology; Social anthropology; Combinations between the social science subjects; and combinations of social science subjects with ''Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
Arts include: subjects under groups 'Language, literature and area studies' and 'Arts other than languages" (History; Archaeology; Philosophy; Theology; Art and design; Drama; Music; 'Arts general, and combined other arts subjects').
Pure Science includes: Biology; Botany; Zoology; Physiology and/or Anatomy; Biochemistry; Other, general or combined biological sciences; Mathematics; mathematics with Physics; Chemistry; Geology; Environmental Sciences (other than geology); Other, general and combined physical sciences; Combinations of biological and physical sciences; Combinations of 'Pure Science' with 'Social, administrative and business studies', 'Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
All Science includes: subjects under groups 'Pure science'; 'Medicine, dentistry and health'; Engineering and technology', 'Agriculture, forestry and veterinary science'
Totals include: all undergraduate students studying for "First degree" *and* "First diploma" *and* "Courses not leading to a qualification". The majority of students who entered higher education in this period studied for a first degree.

	% sociology of social science UG students	% sociology of all UG students	% economics of social science UG students	% economics of all UG students	% social science of all UG students	% social science of 'Arts and social science' UG students	% arts of all UG students	% arts of 'Arts and Social Science' UG students	% arts and social science of all UG students	% pure science of all UG students	% all science of all UG students
1960	-	-	-	-	11.3	26.8	30.8	73.2	42.1	23.8	57.9
1961	-	-	-	-	11.2	26.6	30.9	73.4	42.1	24.6	57.9
1962	-	-	-	-	11.7	27.6	30.7	72.4	42.4	26.0	57.6
1963	-	-	-	-	12.5	29.3	30.1	70.7	42.6	26.4	57.4
1964	-	-	-	-	13.0	29.3	31.3	70.7	44.3	25.6	55.7
1965	-	-	-	-	15.8	36.1	28.0	63.9	43.8	22.5	54.9
1966	8.5	1.8	10.2	2.2	21.6	51.2	20.6	48.8	42.2	25.7	56.2
1967	8.7	1.8	11.7	2.5	21.1	49.5	21.6	50.5	42.7	26.0	55.7
1968	9.5	2.0	11.8	2.5	21.0	48.6	22.3	51.4	43.3	25.6	55.1
1969	9.8	2.0	12.4	2.5	20.5	46.2	23.8	53.8	44.3	24.3	53.9

Table 5. Distribution of students by subject, 1960-9. *Sources*: University Grants Committee. 1960/1-1965/6. *Returns from Universities and University Colleges*. London: HMSO; and University Grants Committee. 1966-69. *Statistics of Education, vol. 6 Universities*. London: HMSO.

Notes:
The figures for % change of sociology and economics students of all UG students is for the period 1966-1978.
Social Science includes: Business management studies; Economics; Geography; Accountancy; Government and public administration; Law; Psychology; Sociology; Social anthropology; Combinations between the social science subjects; and combinations of social science subjects with ''Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
Arts include: subjects under groups 'Language, literature and area studies' and 'Arts other than languages" (History; Archæology; Philosophy; Theology; Art and design; Drama; Music; 'Arts general, and combined other arts subjects').
Pure Science includes: Biology; Botany; Zoology; Physiology and/or Anatomy; Biochemistry; Other, general or combined biological sciences; Mathematics; mathematics with Physics; Chemistry; Geology; Environmental Sciences (other than geology); Other, general and combined physical sciences; Combinations of biological and physical sciences; Combinations of 'Pure Science' with 'Social, administrative and business studies', 'Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
All Science includes: subjects under groups 'Pure science'; 'Medicine, dentistry and health'; Engineering and technology', 'Agriculture, forestry and veterinary science'
Totals include: all undergraduate students studying for "First degree" *and* "First diploma" *and* "Courses not leading to a qualification". The majority of students who entered higher education in this period studied for a first degree.

	% sociology of social science UG students	% sociology of all UG students	% economics of social science UG students	% economics of all UG students	% social science of all UG students	% social science of 'Arts and social science' UG students	% arts of all UG students	% arts of 'Arts and Social Science' UG students	% arts and social science of all UG students	% pure science of all UG students	% all science of all UG students
1970	9.6	2.0	13.0	2.7	20.7	46.4	23.8	53.6	44.5	24.5	54.0
1971	9.1	1.9	12.8	2.7	21.2	47.9	23.0	52.1	44.2	25.2	54.1
1972	8.8	1.9	11.6	2.5	21.6	48.7	22.7	51.3	44.3	25.4	54.2
1973	9.2	2.0	11.3	2.5	22.1	49.1	22.9	50.9	45.0	24.9	53.4
1974	8.5	1.9	11.2	2.5	22.7	49.5	23.2	50.5	45.9	24.3	52.4
1975	8.0	1.9	11.0	2.6	23.6	50.3	23.3	49.7	47.0	23.4	51.4
1976	7.9	1.9	11.2	2.7	24.1	50.8	23.3	49.2	47.5	22.7	50.9
1977	7.4	1.8	11.3	2.7	23.9	50.4	23.5	49.6	47.4	22.7	50.9
1978	7.0	1.7	10.9	2.6	23.9	50.3	23.5	49.7	47.4	22.8	51.0
% Change 1960-78	**-1.5**	**-0.1**	**0.7**	**0.4**	**12.6**	**23.5**	**-7.3**	**-23.5**	**5.3**	**-1.0**	**-6.9**

Table 5 (continued). Distribution of students by subject, 1970-8. *Source*: University Grants Committee. 1970-8. *Statistics of Education, vol. 6 Universities*. London: HMSO.

Notes:
The figures for % change of sociology and economics students of all UG students is for the period 1966-1978.
Social Science includes: Business management studies; Economics; Geography; Accountancy; Government and public administration; Law; Psychology; Sociology; Social anthropology; Combinations between the social science subjects; and combinations of social science subjects with ''Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
Arts include: subjects under groups 'Language, literature and area studies' and 'Arts other than languages" (History; Archaeology; Philosophy; Theology; Art and design; Drama; Music; 'Arts general, and combined other arts subjects').
Pure Science includes: Biology; Botany; Zoology; Physiology and/or Anatomy; Biochemistry; Other, general or combined biological sciences; Mathematics; mathematics with Physics; Chemistry; Geology; Environmental Sciences (other than geology); Other, general and combined physical sciences; Combinations of biological and physical sciences; Combinations of 'Pure Science' with 'Social, administrative and business studies', 'Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
All Science includes: subjects under groups 'Pure science'; 'Medicine, dentistry and health'; Engineering and technology', 'Agriculture, forestry and veterinary science'
Totals include: all undergraduate students studying for "First degree" *and* "First diploma" *and* "Courses not leading to a qualification". The majority of students who entered higher education in this period studied for a first degree.

	% sociology first entrants of social science first entrants	% sociology first entrants of all first entrants	% economics first entrants of social science first entrants	% economics first entrants of all first entrants	% social science first entrants of all first entrants	% social science first entrants of 'Arts and Social Science' first entrants	% art first entrants of all first entrants	% art first entrants of 'Arts and Social Science' first entrants	% Art and Social Science first entrants of all entrants	% pure science first entrants of all first entrants	% all science first entrants of all first entrants
1966	7.4	1.7	7.0	1.6	22.9	52.2	21.0	47.8	43.8	27.8	54.7
1967	7.5	1.6	8.4	1.9	22.1	50.3	21.8	49.7	43.8	28.7	54.8
1968	7.9	1.7	9.3	2.0	21.7	48.3	23.2	51.7	44.9	27.9	53.9
1969	8.1	1.7	10.0	2.1	21.4	45.8	25.4	54.2	46.7	25.8	51.9
1970	7.7	1.6	10.9	2.3	21.2	45.8	25.1	54.2	46.3	26.3	52.4
1971	7.3	1.6	11.5	2.5	21.6	47.7	23.6	52.3	45.1	26.5	53.4
1972	7.1	1.6	10.0	2.3	22.8	49.0	23.7	51.0	46.4	26.5	52.2
1973	7.5	1.8	10.4	2.5	23.9	49.5	24.3	50.5	48.2	25.2	50.3
1974	7.1	1.7	11.0	2.7	24.0	49.1	24.9	50.9	48.9	24.5	49.6
1975	7.0	1.7	11.5	2.8	24.7	49.5	25.1	50.5	49.8	23.5	48.8
1976	6.7	1.7	10.9	2.7	24.6	49.8	24.8	50.2	49.4	23.4	49.2
1977	6.7	1.6	10.3	2.4	23.9	48.7	25.1	51.3	49.0	23.5	49.5
1978	6.3	1.5	10.0	2.4	24.0	48.9	25.0	51.1	49.0	23.9	49.5
% Change	**-1.1**	**-0.2**	**3.0**	**0.8**	**1.1**	**-3.2**	**4.1**	**3.2**	**5.2**	**-3.9**	**-5.2**

Table 6. Distribution of first entrants by subject, 1966-1978. *Source*: University Grants Committee. 1966-78. *Statistics of Education, vol. 6 Universities*. London: HMSO.

Notes:
Social Science includes: Business management studies; Economics; Geography; Accountancy; Government and public administration; Law; Psychology; Sociology; Social anthropology; Combinations between the social science subjects; and combinations of social science subjects with ‘‘Architecture and other professional and vocational subjects’, ‘Language, Literature and area studies’ and ‘Arts other than languages’.
Arts include: subjects under groups ‘Language, literature and area studies’ and ‘Arts other than languages” (History; Archaeology; Philosophy; Theology; Art and design; Drama; Music; ‘Arts general, and combined other arts subjects’).
Pure Science includes: Biology; Botany; Zoology; Physiology and/or Anatomy; Biochemistry; Other, general or combined biological sciences; Mathematics; mathematics with Physics; Chemistry; Geology; Environmental Sciences (other than geology); Other, general and combined physical sciences; Combinations of biological and physical sciences; Combinations of ‘Pure Science’ with ‘Social, administrative and business studies’, ‘Architecture and other professional and vocational subjects’, ‘Language, Literature and area studies’ and ‘Arts other than languages’.
All Science includes: subjects under groups ‘Pure science’; ‘Medicine, dentistry and health’; Engineering and technology’, ‘Agriculture, forestry and veterinary science’
Totals include: all undergraduate students studying for “First degree” **and** “First diploma” **and** “Courses not leading to a qualification”. The majority of students who entered HE in this period studied for a first degree.

	Sociology / social science	Sociology / all	Economics / social science	Economics / all	Social science /all	Social science/ 'Arts and Social Science'	Art/ 'Arts and Social Science'	Art/ all	'Arts and Social Science' / all	Pure science / all	All science/ all
%UG, 1960-78	-1.5	-0.1	0.7	0.4	12.6	23.5	-23.5	-7.3	5.3	-1.0	-6.9
% First entrants, 1960-78	-1.1	-0.2	3.0	0.8	1.1	-3.2	3.2	4.1	5.2	-3.9	-5.2

Table 7. Differences in the distributions of undergraduate students and first entrants by subject, 1960-1978. *Sources*: University Grants Committee. 1960/1-1965/6. *Returns from Universities and University Colleges*. London: HMSO; and University Grants Committee. 1966-78. *Statistics of Education, vol. 6 Universities*. London: HMSO.

Notes:
Social Science includes: Business management studies; Economics; Geography; Accountancy; Government and public administration; Law; Psychology; Sociology; Social anthropology; Combinations between the social science subjects; and combinations of social science subjects with ''Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
Arts include: subjects under groups 'Language, literature and area studies' and 'Arts other than languages" (History; Archaeology; Philosophy; Theology; Art and design; Drama; Music; 'Arts general, and combined other arts subjects').
Pure Science includes: Biology; Botany; Zoology; Physiology and/or Anatomy; Biochemistry; Other, general or combined biological sciences; Mathematics; mathematics with Physics; Chemistry; Geology; Environmental Sciences (other than geology); Other, general and combined physical sciences; Combinations of biological and physical sciences; Combinations of 'Pure Science' with 'Social, administrative and business studies', 'Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
All Science includes: subjects under groups 'Pure science'; 'Medicine, dentistry and health'; Engineering and technology', 'Agriculture, forestry and veterinary science'

	1938-9			1947-8			1951-2			1955-6			1959-60			1962-3		
	Men	**Women**	**Total**	**Men**	**Women**	**Total**	**Men**	**Women**	**Total**	**Men**	**Women**	**Total**	**Men**	**Women**	**Total**	**Men**	**Women**	**Total**
Sociology group	21	12	33	27	63	90	64	56	120	34	84	118	83	169	252	140	201	341
Economics	185	31	216	437	92	529	484	36	520	579	112	691	792	93	885	896	98	994
Politics	41	10	51	117	24	141	168	32	200	87	20	107	147	32	179	173	29	202
TOTAL Social Science	247	53	300	581	179	760	716	124	840	700	216	916	1022	294	1316	1209	328	1537

Table 8. Number of students obtaining an honours degree in social science in British universities. *Source*: Heyworth, L. 1965. *Report of the Committee on Social Studies*. Cmnd. 2660. London: HMSO.

Notes:
Sociology group includes: Sociology, Social Administration and Anthropology.
Economics includes also: Accountancy, Agricultural Economics and Business studies.

	1938/9	1962/3
Sociology group	11%	22.2%
Economics	72%	64.7%
Other	17%	13.1%
TOTAL	100%	100%

Table 9. Percentages of Sociology group honours degrees and Economics honours degrees of all Social Science honours degrees in in British Universities in 1938/9 and 1962/3. *Source*: Heyworth, L. 1965. *Report of the Committee on Social Studies.* Cmnd. 2660. London: HMSO.

	1966-7	1967-8	1968-9	1969-70	1970-1	1972-3
% Arts staff of total staff	21.4	21.3	21.2	21.2	21.3	21.2
% Social Science staff of total staff	13.9	14.3	14.6	14.9	15.5	16.4
% Arts and Social Science staff of total staff	35.3	35.6	35.8	36.1	36.8	37.6
% Social Science staff of 'Arts and Social Science' staff	39.4	40.2	40.8	41.3	42.1	43.6
% Pure Science staff of total staff	32.2	28.4	28.8	28.2	28.3	29.7
% All Science staff of total staff	64.5	64.4	64.2	63.9	63.2	62.4

Table 10. Distribution of staff by subject area. *Source*: University Grants Committee. 1966-72. *Statistics of Education, vol. 6 Universities*. London: HMSO.

Notes:
No separate statistics for sociology staff are available.
No statistics are available for 1971.
Arts include: Education; Language, literature and area studies; Arts other than languages.
All Science includes: Medicine, dentistry and health; Engineering, Technology, Architecture and other professional and vocational subjects; Agriculture, forestry and veterinary science; Pure science.

	1973-4	1974-5	1975-6	1976-7	1977-8	1978-9
% Arts staff of total staff	21.3	21.4	20.9	20.3	20.6	20.5
% Social Science staff of total staff	16.6	17.1	17.2	17.1	17.2	17.3
% Arts and Social Science staff of total staff	37.9	38.5	38.0	37.3	37.8	37.8
% Social science staff of 'Arts and Social Science' staff	43.7	44.4	45.1	45.7	45.5	45.8
% Pure Science staff of total staff	28.9	28.2	27.8	27.5	27.1	26.5
% All Science staff of total staff	62.1	61.5	62.0	61.8	62.2	62.1

Table 10 (continued). Distribution of staff by subject area. *Source*: University Grants Committee. 1973-78. *Statistics of Education, vol. 6 Universities*. London: HMSO.

Notes:
No separate statistics for sociology staff are available.
No statistics are available for 1971.
Arts include: Education; Language, literature and area studies; Arts other than languages.
All Science includes: Medicine, dentistry and health; Engineering, technology, architecture and other professional and vocational subjects; Agriculture, forestry and veterinary science; Pure science.

% Arts staff of total staff	% Social Science staff of total staff	% Arts and Social Science staff of total staff	% Social science staff of 'Arts and Social Science' staff	% Pure Science staff of total staff	% All Science staff of total staff
-0.9	3.4	2.5	6.4	-5.7	-2.4

Table 11. Difference in the distribution of staff by subject area 1966/7-1978/9. *Source*: University Grants Committee. 1966-78. *Statistics of Education, vol. 6 Universities*. London: HMSO.

	1945	1952	1954	1962	1966	1968	1979	1989
Universities teaching sociology first degrees	1			10				45
Universities teaching sociology courses	1		11-14			at least 28	70-75	
Universities teaching sociology and/or social anthropology courses		19			46			

Table 12. Universities teaching sociology first degrees and sociology and social anthropology courses. *Sources*: Abbott (1969); Banks (1954); Fincham (1975); Review Committee on Sociology (1989); Peel (1968); Wakeford (1979); Unesco (1953).

	1966-7			1967-8			1968-9			1969-70		
	Male	**Female**	**Total**	**Male**	**Female**	**Total**	**Male**	**Female**	**Total**	**Male**	**Female**	**Total**
Science	11278	3988	15266	12655	4192	16847	12641	4419	17060	12266	4327	16593
%	73.9%	26.1%		75.1%	24.9%		74.1%	25.9%		73.9%	26.1%	
Economics	793	89	882	953	137	1090	1097	131	1228	1206	172	1378
%	89.9%	10.1%		87.4%	12.6%		89.3%	10.7%		87.5%	12.5%	
Arts	5197	5707	10904	5786	6500	12286	6215	7091	13306	6899	8357	15256
%	47.7%	52.3%		47.1%	52.9%		46.7%	53.3%		45.2%	54.8%	
Sociology	353	577	930	388	579	967	409	635	1044	429	686	1115
%	38.0%	62.0%		40.1%	59.9%		39.2%	60.8%		38.5%	61.5%	

Table 13. First entrant male and female full-time undergraduate students in the UK by subject area, 1966-9. *Source*: University Grants Committee. 1966-9. *Statistics of Education, vol. 6 Universities*. London: HMSO.

Notes:
The data have been extracted from tables on the "Subject and type of study of full-time students at undergraduate level who entered for the first time in…" (title of table may vary slightly across years).
Science includes: Biology; Botany; Zoology; Physiology and/or Anatomy; Biochemistry; Other, general or combined biological sciences; Mathematics; mathematics with Physics; Chemistry; Geology; Environmental Sciences (other than geology); Other, general and combined physical sciences; Combinations of biological and physical sciences; Combinations of 'Pure Science' with 'Social, administrative and business studies', 'Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
Arts include: subjects under groups 'Language, literature and area studies' and 'Arts other than languages" (History; Archaeology; Philosophy; Theology; Art and design; Drama; Music; 'Arts general, and combined other arts subjects').
Totals include: all students studying for "First degree" and "First diploma" and "Courses not leading to a qualification", although, the majority of students who entered HE in this period studied for a first degree.

	1970-1			1971-2			1972-3			1973-4		
	Male	**Female**	**Total**	**Male**	**Female**	**Total**	**Male**	**Female**	**Total**	**Male**	**Female**	**Total**
Science	12614	4611	17225	12970	4844	17814	12644	4908	17552	11966	4955	16921
%	73.2%	26.8%		72.8%	27.2%		72.0%	28.0%		70.7%	29.3%	
Economics	1286	228	1514	1401	264	1665	1241	257	1498	1308	358	1666
%	84.9%	15.1%		84.1%	15.9%		82.8%	17.2%		78.5%	21.5%	
Arts	6836	8457	15293	6778	8717	15495	6562	8775	15337	6805	9050	15855
%	44.7%	55.3%		43.7%	56.3%		42.8%	57.2%		42.9%	57.1%	
Sociology	409	668	1077	407	658	1065	373	698	1071	435	760	1195
%	38.0%	62.0%		38.2%	61.8%		34.8%	65.2%		36.4%	63.6%	

Table 13 (continued). First entrant male and female full-time undergraduate students in the UK by subject area, 1970-3. *Source*: University Grants Committee. 1970-3. *Statistics of Education, vol. 6 Universities*. London: HMSO.

Notes:
The data have been extracted from tables on the "Subject and type of study of full-time students at undergraduate level who entered for the first time in 1966-67" (title of table may vary slightly across years).
Science includes: Biology; Botany; Zoology; Physiology and/or Anatomy; Biochemistry; Other, general or combined biological sciences; Mathematics; mathematics with Physics; Chemistry; Geology; Environmental Sciences (other than geology); Other, general and combined physical sciences; Combinations of biological and physical sciences; Combinations of 'Pure Science' with 'Social, administrative and business studies', 'Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
Arts include: subjects under groups 'Language, literature and area studies' and 'Arts other than languages" (History; Archaeology; Philosophy; Theology; Art and design; Drama; Music; 'Arts general, and combined other arts subjects').
Totals include: all students studying for "First degree" and "First diploma" and "Courses not leading to a qualification", although, the majority of students who entered HE in this period studied for a first degree.

	1974-5			1975-6			1976-7			1977-8			1978-9		
	Male	**Female**	**Total**	**Male**	**Female**	**Total**	**Male**	**Female**	**Total**	**Male**	**Female**	**Total**	**Male**	**Female**	**Total**
Science	12142	5174	17316	12414	5159	17573	12756	5366	18122	13256	5798	19054	13771	6052	19823
%	70.1%	29.9%		70.6%	29.4%		70.4%	29.6%		69.6%	30.4%		69.5%	30.5%	
Economics	1524	353	1877	1710	409	2119	1655	426	2081	1604	376	1980	1545	438	1983
%	81.2%	18.8%		80.7%	19.3%		79.5%	20.5%		81.0%	19.0%		77.9%	22.1%	
Arts	6974	9616	16590	7485	10370	17855	7537	10666	18203	7688	10981	18669	7497	11356	18853
%	42.0%	58.0%		41.9%	58.1%		41.4%	58.6%		41.2%	58.8%		39.8%	60.2%	
Sociology	416	787	1203	485	806	1291	441	838	1279	455	829	1284	431	823	1254
%	34.6%	65.4%		37.6%	62.4%		34.5%	65.5%		35.4%	64.6%		34.4%	65.6%	

Table 13 (continued). First entrant male and female full-time undergraduate students in the UK by subject area, 1974-8. *Source*: University Grants Committee. 1974-8. *Statistics of Education, vol. 6 Universities*. London: HMSO.

Notes:
The data have been extracted from tables on the "Subject and type of study of full-time students at undergraduate level who entered for the first time in 1966-67" (title of table may vary slightly across years).
Science includes: Biology; Botany; Zoology; Physiology and/or Anatomy; Biochemistry; Other, general or combined biological sciences; Mathematics; mathematics with Physics; Chemistry; Geology; Environmental Sciences (other than geology); Other, general and combined physical sciences; Combinations of biological and physical sciences; Combinations of 'Pure Science' with 'Social, administrative and business studies', 'Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
Arts include: subjects under groups 'Language, literature and area studies' and 'Arts other than languages" (History; Archaeology; Philosophy; Theology; Art and design; Drama; Music; 'Arts general, and combined other arts subjects').
Totals include: all students studying for "First degree" and "First diploma" and "Courses not leading to a qualification", although, the majority of students who entered HE in this period studied for a first degree.

	Science	Economics	Arts	Sociology
Male	72%	83.5%	43.6%	36.9%
Female	28%	16.5%	56.4%	63.1%

Table 14. Average % of first entrant male and female full-time undergraduate students in the UK by subject area, 1966-78. *Source*: University Grants Committee. 1966-78. *Statistics of Education, vol. 6 Universities*. London: HMSO.

University	Provision of statistics	Indicator of statistics research/teaching
Birmingham	Yes	'all students have to complete a course in elementary statistics'
Edinburgh	Yes	'calculating machinery is available', 'department is collaborating in the Third Statistical Account of Scotland'
Exeter	Yes	Teaches LSE external degree
Glasgow	Yes	'survey of the Scottish churches', 'work is being done on the Third Statistical Account of Scotland'
Hull	Yes	Teaches LSE external degree
Leeds	No	
Leicester	Yes	Teaches LSE external degree
London	Yes	'wide range of calculating machinery', Department of Sociological and Demographic Research, research on various aspects of social stratification and social mobility.
Manchester	No	
Nottingham	No	
Oxford	Yes	At graduate level – 'The lecturer in sociology is responsible for the Oxford Pilot Social Study', 'use of assistants, computers and interviewers and calculating machinery'
Reading	No	
St Andrews	Yes	'work is being done on the Third Statistical Account of Scotland'
Sheffield	Yes	'work in the social surveys field'
Swansea	Yes	'pilot surveys'

Table 15. Provision of statistical training by type in British universities offering courses in sociology in 1951. *Source*: MacRae, D.G. (1953, [1951]) "The teaching of Sociology, Social Anthropology and Social Psychology" in *The Teaching of the Social Sciences in the United Kingdom.* Geneva: UNESCO. pp. 83-88

Substantive		Methodological		
		Courses on the philosophy of methods or science	Courses that contain practical work on methods	
1	**2***	**3**	**4**	**5**
Courses discussing theoretical issues in an abstract way; these are courses about general principles and theory *per se*; philosophy or history; they approach the study of society from a theoretical/ historical/ philosophical perspective and attempt to teach this approach.	Courses that may take a theoretical approach towards the study of society, but focus substantively on contemporary and specific problems/issues in society. These courses teach *about* such problems and, by looking at the relevant published work, may also teach about how established scholars have empirically investigated such problems; but these courses don't teach methodology as such and therefore, they don't teach empirical skills – they only provide the substantive material to which empirical skills can be applied.	Courses that teach about the philosophy of science and scientific methods; they do not include practical work with method but discuss the epistemological issues related to methods.	All non-statistical methods courses that involve practical work with methods, including interviews, documentary analysis, observations etc. Today, we call these courses "qualitative methods" but this term wasn't used to describe them in the period under investigation.	Statistical courses, including demography. These are the courses, listed under "Statistics" that are recommended to sociologists and are therefore, part of the sociology degree. Demography has been included since statistics is the primary method used in it.
Examples: The Family; Comparative Social institutions; Social Philosophy; Comparative Ethics and Religion; The Scope of Sociology; Greco-Roman Civilisation;	**Examples:** The Social Structure of the UK; Industrial Sociology; Social Policy; The Education Services, The Sociology of Medical Care, The Penal System.	**Examples:** Logic; Logic and Scientific Method	**Examples:** The Local Social Survey; Methods of Social Investigation; Sociological Research	**Examples:** Methods of Social Investigation, Statistical Methods (Sociology); Population Trends and Policy

Table 16. Categories of sociology courses, 1904-1979. *Source*: London School of Economics and Political Science. 1904-1979. *LSE Calendars*. London: LSE.

*It could be argued that some of these courses taught some empirical skills and I have not excluded the possibility of this being the case. However, when I examined their syllabus descriptions I saw no clear sign of this.

Courses available to sociology students	Courses available to all economics students, regardless of their special subject	Courses only available to economics students specialising in statistics
1	**2 and 4**	**3**
This is always clearly indicated in the description of the courses. These courses are not always restricted to sociology students.	Statistics courses available to economics students in first and second year; and statistics courses available to economics students specialising in subjects *other than* statistics in their third year both come category (2). Statistics courses which were available to students specialising in statistics, but were *not restricted to them*, are analysed in category (4).	This is usually clearly indicated in the description of the courses. However, from 1972 onwards, the description of the courses changed from, for instance, "Part II – Special Subject of Statistics" to, for instance, "For B.Sc. (Econ.) Part II; B.Sc. c.u. main fields Maths., Stats., Comp., Maths. and Phil. 2nd yr.; Dip. Stats.; M.Sc. prelim. yr." For the period 1972-1979, I treat as belonging to this category only those courses which provided course units for students specialising in main field statistics. If the description did not mention statistics, but mentioned *only* mathematics and/or computing as main fields, then the course has also been treated as belonging to this category, since I have assumed that fields of specialisation such as mathematics and computing would not have required less specialisation skills than main field statistics.

Table 17. Categories of statistics courses, 1895-1979. *Source*: London School of Economics and Political Science. 1895-1979. *LSE Calendars*. London: LSE.

		1904-1979		1904-1923		1924-1952		1953-1979	
	Numerical category	**Raw Count**	**%**	**Raw Count**	**%**	**Raw Count**	**%**	**Raw Count**	**%**
Theoretical/philosophical/historical courses	1	128	59.8%	38	80.9%	47	58%	66	52.4%
Courses on contemporary issues	2	23	10.7%	7	14.9%	7	8.6%	11	8.7%
Courses on contemporary issues (from SSaA list)	2	39	18.2%	0	0%	13	16%	35	27.8%
Courses on the philosophy of methodology and/or science	3	4	1.9%	1	2.1%	3	3.7%	2	1.6%
Non-statistical methods courses	4	6	2.8%	1	2.1%	4	4.9%	2	1.6%
Statistical courses	5	14	6.5%	0	0%	7	8.6%	10	7.9%
TOTAL		214	100%	47	100%	81	100%	126	100%

Table 18. Sociology courses at the LSE by type and period. *Source*: London School of Economics and Political Science. 1904-1979. *LSE Calendars*. London: LSE.

		1912-1979		1912-1950		1950-1979	
	Numerical category	**Raw Count**	**%**	**Raw Count**	**%**	**Raw Count**	**%**
Theoretical/philosophical/historical courses for SSaA only	1	32	18.5%	27	27.6%	5	6.2%
Theoretical/philosophical/historical courses for SSaA and Sociology	1	8	4.6%	0	0%	8	9.9%
Courses on contemporary issues for SSaA only	2	77	44.5%	57	58.2%	24	29.6%
Courses on contemporary issues for SSaA and Sociology	2	39	22.5%	2	2%	37	45.7%
Non-statistical methods courses	3	7	4%	4	4.1%	3	3.7%
Statistical courses	4	3	1.7%	2	2%	1	1.2%
Medical courses	5	7	4%	6	6.1%	3	3.7%
TOTAL		173	100%	98	100%	81	100%

Table 19. SSaA courses at the LSE by type and period, 1912-1979. *Source*: London School of Economics and Political Science. 1912-1979. *LSE Calendars*. London: LSE.

	Numerical category	Raw count	%
Courses for sociology students (may also be taken by economics students)*	1	10	9.1%
Courses for all economics students (including those who will specialise in statistics)	2	57	51.8%
Courses only for economics students specialising in Statistics	3	28	25.5%
Courses that in some years have been offered to all economics students and in other years only to economics students specialising in Statistics	4	15	13.6%
TOTAL		110	100%

Table 20. Statistics courses at the LSE by type and period, 1895-1979. *Source*: London School of Economics and Political Science. 1895-1979. *LSE Calendars*. London: LSE.

* This excludes the demography courses (N=5), which were listed separately from the statistics courses, but which were included under "Statistical courses" in the analysis of sociology courses above.

	Methods courses reported	Methods courses teaching *some* statistics	Separate statistics methods courses
Peel (1968)	28	19	7
%		67.9%	25%
Wakeford (1979)	98	55	20
%		56.1%	20.4%

Table 21. Analysis of methods courses in sociology undergraduate degrees in UK universities in 1967 and 1978 reported in Peel (1968) and Wakeford (1979). *Sources*: Peel (1968); Wakeford (1979).

	Universities reported	Universities offering *at least* one methods course teaching some statistics	Universities offering at least one separate statistics course
Peel (1968)	25	19	7
%		76.0%	28%
Wakeford (1979)	49	37	13
%		75.5%	26.5%

Table 22. Analysis of methods courses in sociology undergraduate degrees in UK universities in 1967 and 1978 reported in Peel (1968) and Wakeford (1979). *Sources*: Peel (1968); Wakeford (1979).

	Universities teaching UG sociology methods courses	Universities teaching UG sociology methods courses (without missing data)	Universities teaching statistics and computing in UG sociology methods courses	Universities teaching surveys and questionnaires in UG sociology methods courses	Universities teaching participant observation in UG sociology methods courses	Universities teaching interviews in UG sociology methods courses	Universities teaching documentary and content analysis in UG sociology methods courses	Universities teaching experiments in UG sociology methods courses	Universities teaching philosophy of science/ methodology in UG sociology methods courses	Universities in which UG sociology methods courses discuss 'positivism'
Peel (1968)	25	25	12 **48%** **(76%)**	23 **92%**	15 **60%**	12 **48%**	11 **44%**	9 **36%**	7 **28%**	1 **4%**
Wakeford (1979)	49	48	26 **54.2%** **(72.9%)**	43 **89.6%**	43 **89.6%**	36 **75%**	28 **58.3%**	19 **39.6%**	37 **77.1%**	17 **35.4%**

Table 23. Analysis of methodological techniques taught in undergraduate sociology methods courses in universities in the UK in 1967 and 1978 reported in Peel (1968) and Wakeford (1979). *Sources:* Peel (1968) and Wakeford (1979).

Notes*:* Percentages in brackets refer to the proportion of universities teaching statistics **both** where there was little engagement with statistics and little sign of practical work; and where the engagement with statistics was more advanced. Percentages that are **not** in brackets refer to only the proportion of universities where the engagement with statistics was advanced.

	Total Number of Universities	Total number of universities (without missing data)	Advanced Statistical Training	Practical Textbooks	Computing
Peel (1968)	25	22	12 54.5%	12 54.5%	2 9.1%
Wakeford (1979)	49	45	26 57.8%	31 67.4%	18 40%

Table 24. Analysis of the details on the statistical courses taught in undergraduate sociology degrees in universities in 1967 and 1978 reported in Peel (1968) and Wakeford (1979). *Sources:* Peel (1968) and Wakeford (1979).

Notes:

Practical statistical textbooks include numerous titles, which could not all be listed here. For specific titles, please look up the relevant syllabuses in Peel (1968) and Wakeford (1979). Decisions whether to consider textbooks as facilitating the teaching of practical skills, or not, have been made on the basis of background knowledge of the most popular of these textbooks. It should be taken into account, however, that it is, ultimately, up to the particular teacher to use textbooks in a way that facilitates the learning of practical statistical skills. So this analysis is limited to assessing the *opportunity* of these textbooks to have been used in a good way.

In 1978, the proportion of universities using practical statistical textbooks exceeds the proportion of universities teaching statistics at an advanced/practical level because, in some cases, the syllabuses comprised only a list of recommended readings but little or no information about what exactly was taught in particular courses – so there are entries where a course is marked as not offering statistical training but also as using practical textbooks.

	1969		1974		1979	
	Statistics	**Methods**	**Statistics**	**Methods**	**Statistics**	**Methods**
Aberdeen		x		x		x
Aston			no info	no info		x
Bath	x	x	x	x	x	x
Belfast	x	x		x		x
Birmingham	x		x		x	x
Birmingham Polytechnic	not in list	not in list	not in list	not in list	x	x
Bradford						
Bristol	x	x	x	x	x	x
Bristol Polytechnic	not in list	not in list	not in list	not in list	no info	no info
Brunel	no info	no info	x	x	x	x
Cambridge	no info	no info	no info	no info	no info	no info
Cambridge CCA&T	not in list	not in list	not in list	not in list		x
City	not in list	not in list		x		x
Durham		x		x		x
East Anglia	x		x	x	x	x
Edinburgh	x	x	x	x	x	x
Essex	**x***	x	**x***		x	x
Exeter	**x***	x	**x***	x	**x***	x
Glasgow		x	x	x		x
Hatfield Polytechnic	not in list	not in list	not in list	not in list	x	x
High Wycombe CHE	not in list	not in list	not in list	not in list	x	x
Hull		x	x	x		x
Ilkley	not in list	not in list	not in list	not in list		
Keele	x	x	x	x	**x***	x
Kent	x			x	x	
Kingston Polytechnic	not in list	not in list	not in list	not in list	x	x
Lancaster	not in list	not in list		x		x
Leeds	x	x	x	x		x
Leicester		x		x		x
Liverpool	x	x	x	x		x
London Bedford	x	x	x	x		x
London Goldsmiths	no info	no info	no info	no info		
LSE	x	x	x	x	x	x
London College (City)	not in list	not in list	not in list	not in list		x
London College (Middlesex)	not in list	not in list	not in list	not in list		x
London College (NELP)	not in list	not in list	not in list	not in list	no info	no info
London College (N Poly)	not in list	not in list	not in list	not in list		
London College (PCL)	not in list	not in list	not in list	not in list		x
London College (S Bank)	not in list	not in list	not in list	not in list	no info	no info
London College (Thames)	not in list	not in list	not in list	not in list	x	x
Loughborough	no info	no info	x		x	x

Table 25. Statistics and methods courses/methods course elements within sociology degree courses at higher education institutions in the UK in 1969, 1974 and 1979. *Sources*: Which University (1969); Which University (1974); Which Degree (1979).

Notes:
Statistics course/course element is marked as present, if the degree syllabus summary mentioned 'stats' or 'social stats' or 'quantitative methods' or 'social survey methods' or 'maths' or 'stats methods' or 'econ & stats methods' or 'demography' or other very similar variations of these.
Methods course/course element is marked as present, if the degree syllabus summary mentioned 'methods of sociol' or 'methods of social res' or 'methodology' or 'methods of social investigation' or other similar variations of these.
Marked with 'x*' is where there is a separate joint degree 'Sociology with statistics' or 'Sociology with quantitative methods'. This is important, as it could be expected that in these degrees there would be a much greater emphasis on statistics. In fact, if we want to be very strict and find out in which universities the statistical training was sufficient to produce statistically literate sociology graduates, it is these universities and these special joint degrees that we have to look at.

	1969		1974		1979	
	Statistics	**Methods**	**Statistics**	**Methods**	**Statistics**	**Methods**
Manchester			x		x	x
Manchester CHE	not in list	not in list	not in list	not in list		x
Manchester Polytechnic	not in list	not in list	not in list	not in list	x	x
Middlesbrough	not in list	not in list	not in list	not in list	no info	no info
Newcastle	x	x	x	x	x	x
Newcastle Polytechnic	not in list	not in list	not in list	not in list	x	x
Northampton (NENE)	not in list	not in list	not in list	not in list		
Nottingham				x	x	
Oxford	not in list	not in list	no info	no info	no info	no info
Plymouth Polytechnic	not in list	not in list	not in list	not in list	x	x
Plymouth (St Mark & St John)	not in list	not in list	not in list	not in list		
Roehampton IHE	not in list	not in list	not in list	not in list	no info	no info
Reading	x	x	x	x	x	x
Salford	x		x		x	x
Sheffield	x	x	x	x	x	x
Southampton	x		x	x	x	x
Stirling		x		x	x	x
Stoke on Trent	not in list	not in list	not in list	not in list		x
Strathclyde	x	x		x		
Sunderland Polytechnic	not in list	not in list	not in list	not in list	x	x
Surrey	not in list	not in list	no info	no info	no info	no info
Sussex		x	x		x	
Twickenham	not in list	not in list	not in list	not in list	no info	no info
Ulster	x		x			x
Wales (Aberystwyth)	x		no info	no info		x
Wales (Bangor)	not in list	not in list		x		
Wales (Cardiff)		x		x	x	x
Wales (Swansea)		x		x		x
Warrington	not in list	not in list	not in list	not in list		x
Warwick	no info	no info	x	x	x	x
Wolverhampton	not in list	not in list	not in list	not in list	no info	no info
Worcester CHE	not in list	not in list	not in list	not in list		x
York		x	x	x	x	x
TOTAL (excluding 'not in list')		41		46		74
TOTAL (excluding 'no info')		36		40		64

Table 25. (continued). Statistics and methods courses/methods course elements within sociology degree courses at higher education institutions in the UK in 1969, 1974 and 1979. *Sources*: Which University (1969); Which University (1974); Which Degree (1979).

Notes:
Statistics course/course element is marked as present, if the degree syllabus summary mentioned 'stats' or 'social stats' or 'quantitative methods' or 'social survey methods' or 'maths' or 'stats methods' or 'econ & stats methods' or 'demography' or other very similar variations of these.
Methods course/course element is marked as present, if the degree syllabus summary mentioned 'methods of sociol' or 'methods of social res' or 'methodology' or 'methods of social investigation' or other similar variations of these.
Marked with 'x*' is where there is a separate joint degree 'Sociology with statistics' or 'Sociology with quantitative methods'. This is important, as it could be expected that in these degrees there would be a much greater emphasis on statistics. In fact, if we want to be very strict and find out in which universities the statistical training was sufficient to produce statistically literate sociology graduates, it is these universities and these special joint degrees that we have to look at.

	1969	1974	1979
Statistics	61.1%	65.0%	51.6%
Methods	69.4%	80.0%	82.8%
Statistics and Methods	38.9%	47.5%	46.9%
ONLY statistics	19.4%	15.0%	4.7%

Table 26. Percentages of universities/higher education institutions offering at least one sociology degree that contains: statistics course/course element; methods course/course element; both statistics and methods course/course element; and only statistics course/course element. *Sources*: Which University (1969); Which University (1974); Which Degree (1979).

	Total number of universities	Total number of universities offering statistics and survey	Proportion of universities offering statistics and survey
Peel (1968)	25	21	84%
Which University (1969)	36	22	61.1%
Wakeford (1979)	48	39	81.3%
Which University (1979)	64	33	51.6%

Table 27. Comparison of the results from the analysis of Peel (1968) and Wakeford (1979), and Which University (1969) and Which Degree (1979). *Sources:* Peel (1968), Wakeford (1979), Which University (1969), Which Degree (1979).

Notes: In order to match the Which University/ Which degree data, the data from Peel (1968) and Wakeford (1979) refers to average of the numbers of universities teaching statistics and computing combined with the number of universities teaching survey methods.

Appendix II

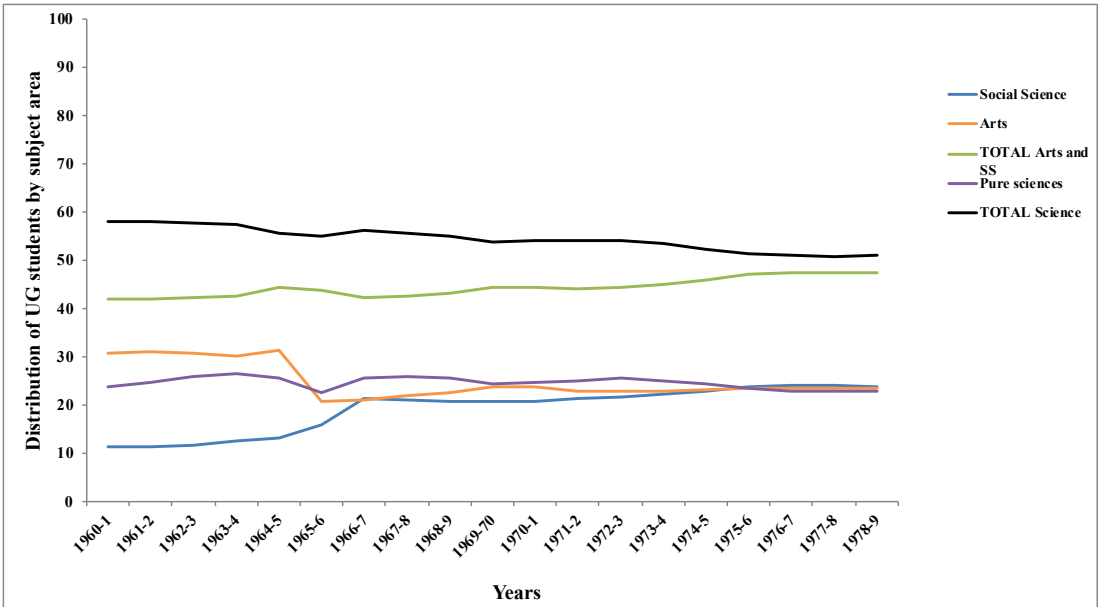

Figure 1. Distribution of undergraduate students by subject, 1960-1979. *Sources*: University Grants Committee. 1960-1965. *Returns from Universities and University Colleges.* London: HMSO; and University Grants Committee. 1966-1978. *Statistics of Education, vol. 6 'Universities'*. London: HMSO.

Notes:

The data in this graph come from two different sources: UGC's *Returns from Universities and University Colleges* and UGC's *Statistics of Education.* Although both sources use very similar categorisation, it is not possible to determine with certainty whether subject categories in both sources are identical, since *Returns from Universities and University Colleges* do not specify what subjects *exactly* come under the categories 'Arts', 'Social Science' and 'Pure Science'. Although it is unlikely that there are great discrepancies between the categorisation employed by the *Returns* and the *Statistics of Education*, the data should be treated with caution.

With regard to the reliability of the categorisation, the *Returns* from 1964 and 1965 mention the following:

'Three of the Scottish universities were unable to make the distinction in 1964-65 and therefore included both arts and social studies under the "Arts" heading. The totals shown under "Social Studies" are therefore understated to some extent and comparisons with previous years should be treated with reserve' (UGC, 1964).

'The proportion of both men and women studying social studies subjects has risen considerably over the five years since 1960-61. However, under the previous system of classification there was a tendency to include students taking combinations of subjects under the 'Arts' heading and it has therefore not been found practicable to determine the exact extent of this increase' (UGC, 1965).

For the period 1966-1978:

Arts include: 'Education', 'Language, Literature and Area studies' and 'Arts other than Languages' (History; Archaeology; Philosophy; Theology; Art and design; Drama; Music)
Social Science includes: Business management studies; Economics; Geography; Accountancy; Government and public administration; Law; Psychology; Sociology; Social anthropology; Combinations between the social science subjects; and combinations of social science subjects with ''Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
Pure Science includes: Biology; Botany; Zoology; Physiology and/or Anatomy; Biochemistry; Other, general or combined biological sciences; Mathematics; mathematics with Physics; Chemistry; Geology; Environmental Sciences (other than geology); Other, general and combined physical sciences; Combinations of biological and physical sciences; Combinations of 'Pure Science' with 'Social, administrative and business studies', 'Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.
All Science includes: subjects under groups 'Pure science'; 'Medicine, dentistry and health'; Engineering and technology', 'Agriculture, forestry and veterinary science'.

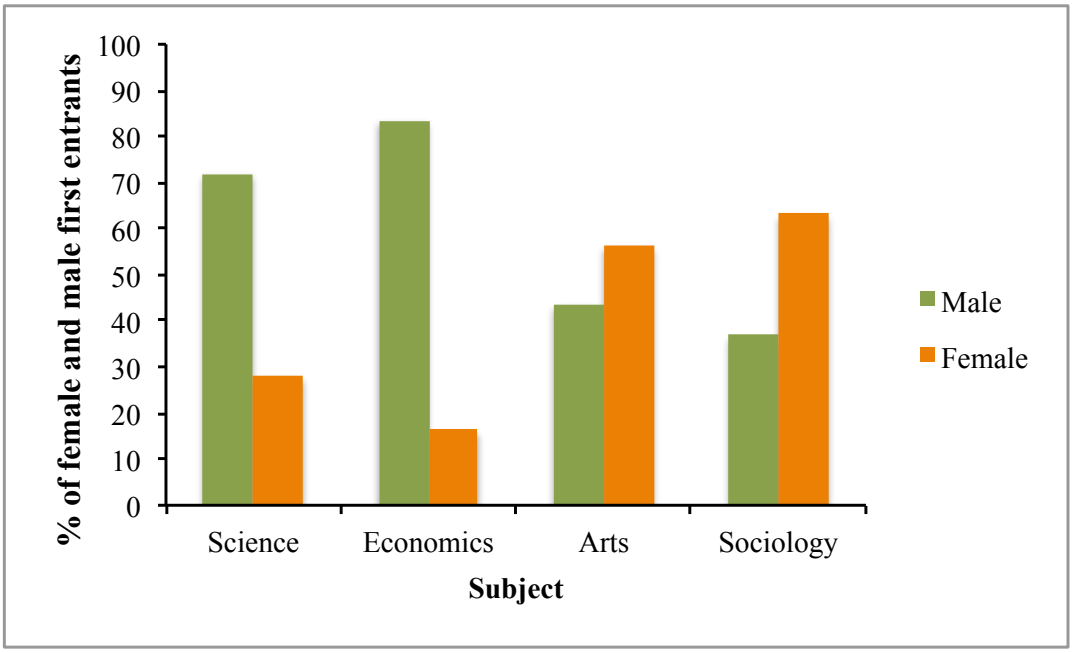

Figure 2. Average percentages of first-entrant male and female full-time undergraduate students in the UK by subject area in the period 1966-1978. *Source*: University Grants Committee. 1966-1978. *Statistics of Education: Vol. 6 Universities.* London: HMSO.

Notes:

Science includes the following subjects: Biology; Botany; Zoology; Physiology and/or Anatomy; Biochemistry; Other, general or combined biological sciences; Mathematics; mathematics with Physics; Chemistry; Geology; Environmental Sciences (other than geology); Other, general and combined physical sciences; Combinations of biological and physical sciences; Combinations of 'Pure Science' with 'Social, administrative and business studies', 'Architecture and other professional and vocational subjects', 'Language, Literature and area studies' and 'Arts other than languages'.

Arts includes the following: subjects under groups 'Language, literature and area studies' and 'Arts other than languages" (History; Archaeology; Philosophy; Theology; Art and design; Drama; Music; 'Arts general, and combined other arts subjects').

Totals include: all students studying for "First degree" **and** "First diploma" **and** "Courses not leading to a qualification", although, the majority of students who entered HE in this period studied for a first degree.

Appendix III

Interviews with Sociologists and Statisticians

To complement my documentary analysis, in the spring of 2017 I interviewed the following sociologists and statisticians:

Bechhofer, F.	*Edinburgh, 7th February 2017.*
Burgess, R.	*London, 9th March 2017.*
Goldstein, H.	*London, 7th March 2017.*
Goldthorpe, J. H.	*Oxford, 1st March 2017.*
MacKenzie, D.	*Edinburgh, 31st January 2017.*
Moore, R.	*Flint, 23rd February 2017.*
Paterson, L.	*Edinburgh, 2nd February 2017.*
Platt, J.	*Brighton, 3rd March 2017.*
Roberts, H.	*London, 4th June 2017.*
Wakeford, J.	*Edinburgh, 20th March 2017.*

These scholars have a long experience in either sociology, statistics or both and were all actively engaged with these subjects in the period between 1960-1980 which is the focus of the third part of this thesis. In the process of selecting possible interviewees, I did not focus on any particular cohort of sociologists or statisticians, as, inevitably, my choice was restricted by the numbers still living and restricted again to those who were able and willing to grant me an interview. My list of potential interviewees also included Martin Bulmer and Ann Oakley who, unfortunately, were not in a position to grant me an interview at the time, although I was able to discuss several issues with them via email.

The interviews were semi-structured: I had a list of questions prepared beforehand to guide me in the interview process, but I also allowed interviewees to comment on issues that arose from our conversation, which they regarded as relevant and important.

There was some variation in the questions depending on the interviewees' profiles; but many of the questions I asked were relevant too all interviewees. I asked all participants about their early experience with sociology: for example, how they first came to sociology; what subjects they studied as part of their degrees; what their impressions of sociology were in those early days; their memories of the expansion and any general comments they might have on the overall post-war development of the subject. I also asked all participants about their experience with methods and methods training or teaching. Where relevant, I asked participants about their experience with learning or teaching quantitative methods and how they perceived the role of these methods in sociology (or, if they could recall, how these methods were taught or perceived more generally back in the 60s, 70s and 80s).

Only those parts of the interviews which included material that I was planning to quote in this thesis were transcribed; the rest of the material from the interviews was summarised for my records. The data from the interviews have been used as an additional support for some of the arguments made in the third part of this thesis; the data have not been used as primary evidence for these arguments, as, inevitably, they provided fascinating but, nonetheless, subjective and memory-based insight into the relationship between sociology and statistics. Overall, the interviews were helpful in suggesting new avenues for documentary examination and providing reassurance that my interpretation of the archival documents corresponded largely with the personal experience and observations of scholars who lived through the period I examine herein.

Many thanks to Professor John H. Goldthorpe for inviting me to Nuffield College's social gathering after the interview and to Professor Robert Moore and Professor Jennifer Platt for accommodating me overnight – your hospitality and generosity made my journey all the more pleasant.

I greatly appreciate the contributions of all interviewees; they breathed life in to this thesis and filled it with the colour of their memories.

Plamena Panayotova

Edinburgh, July 2018

THE UNIVERSITY *of* EDINBURGH

British Sociology and Statistics (1945-1980s)

Interview Consent Form

Plamena Panayotova

PhD SUMMARY

The focus of this research is the relationship between British sociology and statistical methods, as it developed throughout the late nineteenth and twentieth centuries. It examines the question: why sociology in the UK has never succeeded in establishing a firm, enduring relationship with statistical methods, given that statistical methods would appear to offer the prospect of enhancing many areas of sociological research and potentially add a kind of 'scientific' respectability that sociology has often been accused of lacking. My thesis aims to show that British sociology's fraught relationship with statistics, and probabilistic thinking in general, has something very important to tell us about sociology's association with the sciences as a whole, about the culture in which it emerged and about its concept of society.

THE AIM AND COURSE OF THE INTERVIEWS

As part of my research on post-war sociology in the UK and its relationship with statistics, I am conducting a series of interviews with eminent British sociologists who were particularly active in the 1950s-1980s. To-date, I have collected a large amount of documentary evidence on the institutional development of sociology in this period and examined the role of statistics in sociological methodology. I believe, that the interviews will be helpful in providing a first hand perspective and will allow me to enhance, as well as double-check, my own interpretation of the documentary sources. Interviewees may be able to suggest important factors in the relationship between statistics and British sociology not readily found in the literature.

I am asking all participants about their general experience in sociology and/or statistical methods. This will be accompanied by some questions regarding the development of post-war British sociology, if they are relevant to the interviewees' experience and special interests. I am also leaving space for questions that may arise from the interviewees' personal reflections and experience. I expect the interviews to take between one and two hours, depending on how much you want to say.

YOUR RIGHTS

You have the right to stop the interview at any stage or refuse to answer any question. If after the interview, you decide you no longer wish your interview to be used in this research, you have up to two weeks from today to contact me. If you have any questions about the research, please feel free to ask me before we begin the interview.

CONFIDENTIALITY

In referencing any information derived from your interview, would you permit me to use your name, or would you prefer to remain anonymous? Please, indicate your preference as below.

YES, use my name **NO, I prefer to remain anonymous**

Participant's Name (in capitals)

Participant's Signature Date:

Researcher's Name (in capitals)

Researcher's Signature Date: